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THE OBSERVATORY,

A

MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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ANNUAL COMPANION

TO

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 274. 1899. Vol. XXII.

INTRODUCTION.

The present 'Companion' closely resembles that of last year.

As in former years, we have to thank Mr. Denning for revising the "Meteor Notes," and Mr. Maw for kindly supplying a number of observations of Double Stars.

M. Loewy has again favoured us with advance-proofs from which we have obtained the Variable Star Ephemerides. The occultations of stars by the Moon, the phenomena of Jupiter's Satellites, and the diagrams and Ephemerides of the Satellites of the other planets are from the 'Nautical Almanac.'

We are indebted to Mr. Crommelin for the list of stars to be occulted by the Moon (as seen from Greenwich) during the lunar eclipse of December 16, and for other help in preparation.

The "Fraction of the Year" is the fraction which has elapsed at mean noon of the particular day from mean noon on January 1. The Moon's declination is given for mean midnight. The "Longitude of the Moon's Terminator" is given for mean midnight; the letters M. and E. signifying morning and evening—that is, that the Sun is rising or setting on that particular longitude of the Moon's surface; the sign + indicates longitudes reckoned from the central meridian in a westward direction. The angles of disappearance and reappearance for occultations are reckoned from the *true* north in the direction N. E. S. W., as for double stars.

Greenwich Mean Time is used in all cases, and the astronomical day is reckoned from noon to noon as in previous years.

Date.	Day of Year.	Fr. of Year.	Sun		⊙'s Dec. Noon.	M.T. at Apparent Noon.	Sid. T. at Mean Noon.	Moon's Phases.
			Sets.	Rises.				
			h m	h m	° ′	h m s	h m s	d h m
Jan. 1	1	·000	4 0	20 8	23 0 S	0 3 47	18 43 41	☾ 4 15 22
8	8	·019	4 8	20 6	22 14	6 56	19 11 17	● 11 10 50
15	15	·038	4 19	20 0	21 7	9 40	19 38 53	☽ 18 4 36
22	22	·058	4 30	19 53	19 39	11 50	20 6 29	○ 26 7 34
29	29	·077	4 42	19 45	17 54	13 21	20 34 4	
Feb. 5	36	·096	4 54	19 32	15 54	14 13	21 1 40	☾ 3 5 24
12	43	·115	5 6	19 20	13 40	14 27	21 29 16	● 9 21 32
19	50	·134	5 19	19 7	11 15	14 3	21 56 52	☽ 16 20 52
26	57	·153	5 32	18 52	8 41	13 5	22 24 28	○ 25 2 16
Mar. 5	64	·173	5 45	18 36	6 1	11 40	22 52 4	☾ 4 16 7
12	71	·192	5 57	18 21	3 17	9 55	23 19 40	● 11 7 53
19	78	·211	6 10	18 5	0 31 S	7 55	23 47 16	☽ 18 15 24
26	85	·230	6 20	17 50	2 14 N	5 47	0 14 51	○ 26 18 19
April 2	92	·249	6 33	17 34	4 57	3 39	0 42 27	☾ 2 23 56
9	99	·268	6 44	17 18	7 36	0 1 38	1 10 3	● 9 18 21
16	106	·288	6 55	17 2	10 9	23 59 49	1 37 39	☽ 17 10 43
23	113	·307	7 8	16 45	12 33	58 17	2 5 15	○ 25 7 22
30	120	·326	7 19	16 35	14 48	57 8	2 32 51	
May 7	127	·345	7 30	16 22	16 50	56 26	3 0 27	☾ 2 5 47
14	134	·364	7 41	16 11	18 39	56 11	3 28 3	● 9 5 39
21	141	·383	7 50	16 2	20 12	56 24	3 55 38	☽ 17 5 13
28	148	·403	8 0	15 54	21 28	57 2	4 23 14	○ 24 17 49 ☾ 31 10 55
June 4	155	·422	8 8	15 49	22 27	58 33	4 50 50	● 7 18 21
11	162	·441	8 13	15 45	23 6	23 59 22	5 18 26	☽ 15 21 47
18	169	·460	8 17	15 44	23 25	0 0 50	5 46 2	○ 23 2 20
25	176	·479	8 19	15 46	23 24	2 20	6 13 38	☾ 29 16 45
July 2	183	·498	8 17	15 50	23 3	3 45	6 41 14	● 7 8 31
9	190	·518	8 14	15 57	22 22	4 57	7 8 50	☽ 15 11 59
16	197	·537	8 8	16 4	21 22	5 49	7 36 26	○ 22 9 42
23	204	·556	8 0	16 12	20 5	6 15	8 4 2	☾ 29 0 43
30	211	·575	7 49	16 24	18 31	6 13	8 31 37	
Aug. 6	218	·594	7 38	16 35	16 42	5 42	8 59 13	● 5 23 48
13	225	·613	7 25	16 45	14 40	4 42	9 26 49	☽ 13 23 54
20	232	·633	7 11	16 55	12 27	3 15	9 54 25	○ 20 16 45
27	239	·652	6 57	17 7	10 4	0 1 25	10 22 1	☾ 27 11 57
Sept. 3	246	·671	6 42	17 18	7 33	23 59 17	10 49 37	● 4 15 33
10	253	·690	6 25	17 29	4 56	56 56	11 17 13	☽ 12 9 49
17	260	·709	6 10	17 40	2 15 N	54 29	11 44 49	○ 19 0 31
24	267	·728	5 54	17 51	0 29 S	52 1	12 12 25	☾ 26 3 3
Oct. 1	274	·747	5 37	18 3	3 12	49 41	12 40 0	● 4 7 14
8	281	·767	5 22	18 16	5 54	47 35	13 7 36	☽ 11 18 10
15	288	·786	5 6	18 27	8 33	45 50	13 35 12	○ 18 10 5
22	295	·805	4 52	18 38	11 5	44 32	14 2 48	☾ 25 21 40
29	302	·824	4 37	18 51	13 29	43 48	14 30 24	
Nov. 5	309	·843	4 25	19 4	15 43	43 42	14 58 0	● 2 22 27
12	316	·862	4 14	19 16	17 44	44 16	15 25 36	☽ 10 1 35
19	323	·882	4 7	19 28	19 29	45 30	15 53 12	○ 16 22 18
26	330	·901	3 58	19 39	20 58	47 25	16 20 47	☾ 24 18 35
Dec. 3	337	·920	3 52	19 49	22 7	49 57	16 48 23	● 2 12 48
10	344	·939	3 49	19 58	22 56	52 57	17 15 59	☽ 9 9 3
17	351	·958	3 49	20 5	23 22	56 16	17 43 35	○ 16 13 31
24	358	·977	3 52	20 7	23 26	23 59 45	18 11 11	☾ 24 15 57
31	365	·997	3 58	20 8	23 6 S	0 3 11	18 38 47	

For Moon's phases: ● signifies New; ☽ First Quarter; ○ Full; ☾ Last Quarter.

JANUARY.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	**S**	9 4	15 44	5 16 N	+30·6 E.
2	M	10 13	16 25	0 14 N	+18·4 E.
3	Tu	11 24	17 8	4 53 S	+ 6·3 E.
4	W	12 36	17 52	9 56	− 5·9 E.
5	Th	13 51	18 40	14 41	−18·1 E.
6	F	15 10	19 32	18 54	−30·3 E.
7	S	16 29	20 29	22 13	−42·5 E.
8	**S**	17 45	21 31	24 16	−54·7 E.
9	M	18 48	22 35	24 43	−66·9 E.
10	Tu	19 39	23 40	23 35	−79·1 E.
11	W	sets	......	20 22	+89·0 M.
12	Th	5 18	0 41	15 55	+76·9 M.
13	F	6 49	1 39	10 30	+64·7 M.
14	S	8 16	2 33	4 35 S	+52·5 M.
15	**S**	9 40	3 24	1 25 N	+40·3 M.
16	M	11 0	4 13	7 9	+28·2 M.
17	Tu	12 17	5 1	12 22	+16·0 M.
18	W	13 34	5 49	16 50	+ 3·9 M.
19	Th	14 46	6 38	20 25	− 8·3 M.
20	F	15 54	7 28	22 59	−20·5 M.
21	S	16 54	8 18	24 26	−32·7 M.
22	**S**	17 45	9 9	24 43	−44·8 M.
23	M	18 26	9 59	23 53	−57·0 M.
24	Tu	18 58	10 47	21 58	−69·1 M.
25	W	19 23	11 34	19 7	−81·3 M.
26	Th	rises	12 19	15 30	+86·6 E.
27	F	5 47	13 2	11 15	+74·4 E.
28	S	6 55	13 43	6 33	+62·2 E.
29	**S**	8 3	14 25	1 34 N	+50·0 E.

FEBRUARY.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	W	11 37	16 35	13 19 S	+13·7 E.
2	Th	12 52	17 24	17 36	+ 1·5 E.
3	F	14 8	18 17	21 7	−10·7 E.
4	S	15 23	19 14	23 34	−22·8 E.
5	**S**	16 30	20 15	24 39	−35·0 E.
6	M	17 26	21 18	24 8	−47·2 E.
7	Tu	18 9	22 20	21 56	−59·4 E.
8	W	18 40	23 19	18 11	−71·6 E.
9	Th	sets	......	13 12	−83·7 E.
10	F	5 39	0 16	7 26	+84·2 M.
11	S	7 6	1 9	1 19 S	+72·0 M.
12	**S**	8 33	2 1	4 44 N	+59·9 M.
13	M	9 54	2 51	10 20	+47·7 M.
14	Tu	11 13	3 41	15 15	+35·5 M.
15	W	12 30	4 31	19 15	+23·4 M.
16	Th	13 41	5 22	22 11	+11·2 M.
17	F	14 46	6 13	23 59	− 1·0 M.
18	S	15 40	7 4	24 37	−13·2 M.
19	**S**	16 23	7 54	24 5	−25·4 M.
20	M	17 0	8 43	22 28	−37·5 M.
21	Tu	17 27	9 31	19 54	−49·7 M.
22	W	17 50	10 16	16 30	−61·9 M.
23	Th	18 8	11 0	12 25	−74·1 M.
24	F	rises	11 42	7 50	−86·2 M.
25	S	5 54	12 24	2 55 N	+81·7 E.
26	**S**	7 4	13 6	2 11 S	+69·5 E.
27	M	8 14	13 49	7 15	+57·3 E.
28	Tu	9 26	14 34	12 5 S	+45·2 E.

MARCH.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	W	10 41	15 21	16 28 S	+33·0 E.
2	Th	11 56	16 13	20 9	+20·8 E.
3	F	13 10	17 7	22 51	+ 8·6 E.
4	S	14 17	18 5	24 17	− 3·6 E.
5	**S**	15 16	19 5	24 16	−15·7 E.
6	M	16 2	20 5	22 43	−27·9 E.
7	Tu	16 37	21 3	19 39	−40·1 E.
8	W	17 6	22 0	15 18	−52·3 E.
9	Th	17 29	22 54	10 0	−64·5 E.
10	F	sets	23 46	4 6 S	−76·6 E.
11	S	6 0	......	1 58 N	−88·8 E.
12	**S**	7 24	0 37	7 49	+79·0 M.
13	M	8 45	1 28	13 7	+66·8 M.
14	Tu	10 5	2 19	17 34	+54·6 M.
15	W	11 21	3 11	21 0	+42·5 M.
16	Th	12 30	4 3	23 15	+30·3 M.
17	F	13 31	4 56	24 17	+18·1 M.
18	S	14 19	5 47	24 7	+ 5·9 M.
19	**S**	14 57	6 37	22 50	− 6·3 M.
20	M	15 29	7 25	20 33	−18·5 M.
21	Tu	15 53	8 12	17 24	−30·6 M.
22	W	16 13	8 56	13 34	−42·8 M.
23	Th	16 31	9 39	9 9	−55·0 M.
24	F	16 46	10 21	4 21 N	−67·3 M.
25	S	17 2	11 3	0 42 S	−79·4 M.
26	**S**	rises	11 46	5 48	+88·4 E.
27	M	7 13	12 31	10 44	+76·2 E.
28	Tu	8 27	13 18	15 17	+64·1 E.
29	W	9 43	14 9	19 11	+51·9 E.

APRIL.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	S	13 9	16 59	24 9 S	+15·3 E.
2	**S**	14 0	17 58	22 58	+ 3·1 E.
3	M	14 38	18 55	20 21	− 9·0 E.
4	Tu	15 7	19 51	16 29	−21·2 E.
5	W	15 31	20 44	11 38	−33·4 E.
6	Th	15 52	21 35	6 7	−45·6 E.
7	F	16 10	22 25	0 17 S	−57·8 E.
8	S	16 30	23 15	5 31 N	−70·1 E.
9	**S**	sets	......	10 58	−82·2 M.
10	M	7 39	0 6	15 45	+85·6 M.
11	Tu	8 56	0 58	19 36	+73·4 M.
12	W	10 10	1 51	22 19	+61·2 M.
13	Th	11 15	2 44	23 49	+49·1 M.
14	F	12 10	3 37	24 3	+36·9 M.
15	S	12 54	4 29	23 8	+24·7 M.
16	**S**	13 27	5 18	21 9	+12·5 M.
17	M	13 55	6 5	18 16	+ 0·3 M.
18	Tu	14 16	6 50	14 39	−11·9 M.
19	W	14 34	7 33	10 28	−24·1 M.
20	Th	14 51	8 15	5 50	−36·3 M.
21	F	15 7	8 57	0 54 N	−48·5 M.
22	S	15 23	9 40	4 9 S	−60·7 M.
23	**S**	15 41	10 24	9 9	−73·0 M.
24	M	rises	11 11	13 52	−85·2 E.
25	Tu	7 26	12 2	18 0	+82·6 E.
26	W	8 44	12 56	21 16	+70·4 E.
27	Th	9 58	13 53	23 21	+58·2 E.
28	F	11 3	14 53	24 1	+46·0 E.
29	S	11 56	15 53	23 10	+33·8 E.

MAY.

Day. Month.	Day. Week.	Moon. Rises. h m	Moon. Souths. h m	Moon. Dec. ° ′	Moon. Long. of Terminator. °
1	M	13 10	17 47	17 14 S	+ 9·3 E.
2	Tu	13 35	18 40	12 38	− 2·9 E.
3	W	13 56	19 30	7 22	−15·1 E.
4	Th	14 15	20 19	1 45 S	−27·3 E.
5	F	14 35	21 8	3 55 N	−39·5 E.
6	S	14 55	21 57	9 21	−51·7 E.
7	**S**	15 17	22 48	14 14	−63·9 E.
8	M	15 43	23 40	18 20	−76·1 E.
9	Tu	sets	......	21 25	−88·3 E.
10	W	8 59	0 33	23 19	+79·5 M.
11	Th	9 59	1 26	23 58	+67·3 M.
12	F	10 48	2 19	23 25	+55·1 M.
13	S	11 25	3 10	21 45	+42·9 M.
14	**S**	11 55	3 58	19 8	+30·7 M.
15	M	12 18	4 44	15 44	+18·5 M.
16	Tu	12 38	5 28	11 44	+ 6·2 M.
17	W	12 55	6 10	7 16	− 6·0 M.
18	Th	13 12	6 51	2 29 N	−18·2 M.
19	F	13 27	7 33	2 28 S	−30·4 M.
20	S	13 44	8 16	7 26	−42·6 M.
21	**S**	14 4	9 2	12 13	−54·8 M.
22	M	14 27	9 50	16 34	−67·0 M.
23	Tu	14 51	10 43	20 11	−79·3 M.
24	W	rises	11 40	22 44	+88·5 E.
25	Th	8 49	12 41	23 54	+76·3 E.
26	F	9 49	13 42	23 30	+64·1 E.
27	S	10 35	14 43	21 31	+51·9 E.
28	**S**	11 11	15 41	18 8	+39·7 E.
29	M	11 39	16 36	13 40	+27·5 E.
30	Tu	12 2	17 28	8 28	+15·2 E.

JUNE.

Day. Month.	Day. Week.	Moon. Rises. h m	Moon. Souths. h m	Moon. Dec. ° ′	Moon. Long. of Terminator. °
1	Th	12 41	19 5	2 45 N	− 9·3 E.
2	F	13 0	19 54	8 10	−21·5 E.
3	S	13 21	20 43	13 7	−33·7 E.
4	**S**	13 44	21 33	17 21	−45·9 E.
5	M	14 14	22 25	20 39	−58·1 E.
6	Tu	14 52	23 18	22 52	−70·4 E.
7	W	sets	......	23 53	−82·5 E.
8	Th	8 41	0 11	23 41	+85·2 M.
9	F	9 23	1 2	22 21	+73·0 M.
10	S	9 56	1 52	20 0	+60·8 M.
11	**S**	10 21	2 39	16 49	+48·6 M.
12	M	10 42	3 23	12 59	+36·3 M.
13	Tu	11 1	4 6	8 39	+24·1 M.
14	W	11 16	4 47	4 0 N	+11·8 M.
15	Th	11 33	5 28	0 51 S	− 0·4 M.
16	F	11 48	6 10	5 45	−12·6 M.
17	S	12 6	6 53	10 32	−24·8 M.
18	**S**	12 27	7 39	15 0	−37·0 M.
19	M	12 54	8 29	18 52	−49·2 M.
20	Tu	13 29	9 24	21 52	−61·4 M.
21	W	14 16	10 23	23 37	−73·7 M.
22	Th	rises	11 24	23 52	−86·0 M.
23	F	8 27	12 27	22 27	+81·9 E.
24	S	9 9	13 28	19 29	+69·7 E.
25	**S**	9 40	14 27	15 13	+57·5 E.
26	M	10 7	15 21	10 3	+45·2 E.
27	Tu	10 38	16 13	4 23 S	+32·9 E.
28	W	10 47	17 3	1 24 N	+20·7 E.
29	Th	11 6	17 52	6 58	+ 8·5 E.
30	F	11 27	18 41	12 5 N	− 3·7 E.

JULY.

Day. Month.	Day. Week.	Moon. Rises. h m	Moon. Souths. h m	Moon. Dec. ° ′	Moon. Long. of Terminator. °
1	S	11 50	19 30	16 29 N	−15·9 E.
2	**S**	12 18	20 21	19 59	−28·1 E.
3	M	12 51	21 13	22 27	−40·3 E.
4	Tu	13 35	22 5	23 45	−52·5 E.
5	W	14 26	22 57	23 52	−64·7 E.
6	Th	15 27	23 47	22 50	−77·0 E.
7	F	sets	......	20 45	−89·3 E.
8	S	8 26	0 35	17 48	+78·5 M.
9	**S**	8 48	1 20	14 8	+66·3 M.
10	M	9 6	2 3	9 56	+54·1 M.
11	Tu	9 24	2 45	5 23	+41·9 M.
12	W	9 40	3 26	0 36 N	+29·7 M.
13	Th	9 55	4 6	4 14 S	+17·4 M.
14	F	10 11	4 48	8 59	+ 5·2 M.
15	S	10 29	5 32	13 28	− 7·0 M.
16	**S**	10 53	6 19	17 30	−19·2 M.
17	M	11 23	7 10	20 47	−31·4 M.
18	Tu	12 3	8 5	23 1	−43·6 M.
19	W	12 56	9 5	23 56	−55·9 M.
20	Th	14 4	10 7	23 16	−68·2 M.
21	F	15 24	11 9	20 58	−80·4 M.
22	S	rises	12 10	17 11	+87·4 E.
23	**S**	8 8	13 8	12 18	+75·1 E.
24	M	8 31	14 2	6 35	+62·8 E.
25	Tu	8 51	14 55	0 38 S	+50·6 E.
26	W	9 11	15 46	5 13 N	+38·4 E.
27	Th	9 32	16 36	10 38	+26·2 E.
28	F	9 55	17 27	15 20	+14·1 E.
29	S	10 21	18 18	19 8	+ 1·9 E.

AUGUST.

Day. Month.	Day. Week.	Moon. Rises. h m	Moon. Souths. h m	Moon. Dec. ° ′	Moon. Long. of Terminator. °
1	Tu	12 23	20 54	23 53 N	−34·8 E.
2	W	13 21	21 44	23 8	−47·0 E.
3	Th	14 23	22 32	21 20	−59·2 E.
4	F	15 28	23 18	18 37	−71·4 E.
5	S	sets	......	15 9	−83·7 E.
6	**S**	7 14	0 2	11 6	+84·1 M.
7	M	7 31	0 44	6 38	+71·9 M.
8	Tu	7 48	1 25	1 55 N	+59·7 M.
9	W	8 4	2 6	2 53 S	+47·5 M.
10	Th	8 19	2 47	7 38	+35·3 M.
11	F	8 37	3 30	12 9	+23·1 M.
12	S	8 59	4 15	16 14	+10·8 M.
13	**S**	9 24	5 3	19 41	− 1·4 M.
14	M	9 59	5 55	22 14	−13·6 M.
15	Tu	10 44	6 50	23 37	−25·3 M.
16	W	11 42	7 49	23 36	−38·0 M.
17	Th	12 56	8 50	22 3	−50·2 M.
18	F	14 19	9 50	18 59	−62·4 M.
19	S	15 47	10 49	14 35	−74·7 M.
20	**S**	rises	11 46	9 13	−86·9 M.
21	M	6 55	12 40	3 17 S	+80·9 E.
22	Tu	7 14	13 34	2 45 N	+68·7 E.
23	W	7 35	14 26	8 31	+56·5 E.
24	Th	7 58	15 18	13 38	+44·3 E.
25	F	8 25	16 11	17 52	+32·1 E.
26	S	8 56	17 4	21 1	+19·9 E.
27	**S**	9 33	17 5	22 58	+ 7·7 E.
28	M	10 20	18 49	23 42	− 4·5 E.
29	Tu	11 14	19 40	23 15	−16·8 E.
30	W	12 15	20 29	21 44	−29·0 E.

SEPTEMBER.

Day. Month.	Day. Week.	Moon. Rises.	Moon. Souths.	Moon. Dec.	Moon. Long. of Terminator.
		h m	h m	° ′	°
1	F	14 26	22 0	16 0 N	−53·4 E.
2	S	15 33	22 43	12 8	−65·6 E.
3	**S**	16 40	23 24	7 47	−77·8 E.
4	M	sets	……	3 9 N	−89·9 E.
5	Tu	6 11	0 5	1 38 S	+77·8 M.
6	W	6 27	0 47	6 24	+65·6 M.
7	Th	6 45	1 29	10 58	+53·4 M.
8	F	7 4	2 13	15 8	+41·2 M.
9	S	7 29	3 0	18 42	+29·0 M.
10	**S**	8 0	3 50	21 27	+16·8 M.
11	M	8 41	4 44	23 7	+ 4·5 M.
12	Tu	9 33	5 39	23 31	− 7·7 M.
13	W	10 37	6 37	22 32	−19·9 M.
14	Th	11 54	7 36	20 6	−32·1 M.
15	F	13 16	8 33	16 22	−44·3 M.
16	S	14 41	9 29	11 32	−56·5 M.
17	**S**	16 10	10 24	5 57 S	−68·7 M.
18	M	17 37	11 18	0 1 N	−80·9 M.
19	Tu	rises	12 11	5 56	+86·9 E.
20	W	5 59	13 4	11 24	+74·7 E.
21	Th	6 25	13 58	16 6	+62·5 E.
22	F	6 54	14 53	19 45	+50·3 E.
23	S	7 30	15 47	22 11	+38·1 E.
24	**S**	8 15	16 41	23 20	+25·9 E.
25	M	9 7	17 34	23 15	+13·7 E.
26	Tu	10 7	18 24	22 1	+ 1·5 E.
27	W	11 11	19 12	19 48	−10·7 E.
28	Th	12 16	19 57	16 46	−22·9 E.
29	F	13 24	20 40	13 5	−35·1 E.
30	S	14 29	21 22	8 54 N	−47·2 E.

OCTOBER.

Day. Month.	Day. Week.	Moon. Rises.	Moon. Souths.	Moon. Dec.	Moon. Long. of Terminator.
		h m	h m	° ′	°
1	**S**	15 36	22 4	4 21 N	−59·4 E.
2	M	16 44	22 45	0 22 S	−71·6 E.
3	Tu	17 52	23 27	5 8	−83·8 E.
4	W	sets	……	9 46	+84·0 M.
5	Th	5 11	0 12	14 3	+71·8 M.
6	F	5 35	0 58	17 47	+59·6 M.
7	S	6 5	1 48	20 43	+47·4 M.
8	**S**	6 42	2 40	22 37	+35·2 M.
9	M	7 31	3 35	23 18	+23·1 M.
10	Tu	8 31	4 32	22 39	+10·9 M.
11	W	9 42	5 29	20 38	− 1·3 M.
12	Th	11 0	6 25	17 22	−13·5 M.
13	F	12 21	7 19	13 2	−25·7 M.
14	S	13 46	8 12	7 53	−37·9 M.
15	**S**	15 9	9 5	2 15 S	−50·1 M.
16	M	16 34	9 56	3 33 N	−62·3 M.
17	Tu	17 55	10 49	9 7	−74·4 M.
18	W	rises	11 42	14 7	−86·6 M.
19	Th	4 51	12 37	18 13	+81·2 E.
20	F	5 25	13 33	21 10	+69·0 E.
21	S	6 7	14 28	22 50	+56·8 E.
22	**S**	6 56	15 23	23 11	+44·6 E.
23	M	7 54	16 15	22 20	+32·4 E.
24	Tu	8 58	17 5	20 24	+20·3 E.
25	W	10 4	17 52	17 35	+ 8·1 E.
26	Th	11 11	18 36	14 5	− 4·1 E.
27	F	12 18	19 18	10 2	−16·3 E.
28	S	13 23	20 0	5 38	−28·5 E.
29	**S**	14 29	20 41	0 59 N	−40·6 E.
30	M	15 38	21 23	3 45 S	−52·8 E.

NOVEMBER.

Day. Month.	Day. Week.	Moon. Rises.	Moon. Souths.	Moon. Dec.	Moon. Long. of Terminator.
		h m	h m	° ′	°
1	W	17 58	22 53	12 49 S	−77·1 E.
2	Th	sets	23 42	16 44	−89·3 E.
3	F	4 7	……	19 55	+78·5 M.
4	S	4 42	0 34	22 7	+66·3 M.
5	**S**	5 27	1 30	23 7	+54·1 M.
6	M	6 24	2 27	22 45	+42·0 M.
7	Tu	7 32	3 24	21 1	+29·8 M.
8	W	8 48	4 21	18 0	+17·7 M.
9	Th	10 9	5 15	13 57	+ 5·5 M.
10	F	11 30	6 8	9 5	− 6·7 M.
11	S	12 52	6 59	3 43 S	−18·9 M.
12	**S**	14 12	7 49	1 52 N	−31·1 M.
13	M	15 33	8 39	7 20	−43·3 M.
14	Tu	16 53	9 30	12 24	−55·5 M.
15	W	18 14	10 23	16 45	−67·6 M.
16	Th	rises	11 18	20 6	−79·7 M.
17	F	3 57	12 14	22 15	+88·1 E.
18	S	4 44	13 9	23 6	+76·0 E.
19	**S**	5 40	14 4	22 41	+63·8 E.
20	M	6 42	14 55	21 5	+51·6 E.
21	Tu	7 48	15 44	18 32	+39·5 E.
22	W	8 56	16 30	15 13	+27·3 E.
23	Th	10 2	17 13	11 19	+15·2 E.
24	F	11 10	17 55	7 1	+ 3·0 E.
25	S	12 15	18 36	2 28 N	− 9·2 E.
26	**S**	13 22	19 17	2 12 S	−21·3 E.
27	M	14 28	19 59	6 51	−33·5 E.
28	Tu	15 38	20 44	11 20	−45·7 E.
29	W	16 50	21 32	15 25	−57·9 E.
30	Th	18 3	22 23	18 54 S	−70·0 E.

DECEMBER.

Day. Month.	Day. Week.	Moon. Rises.	Moon. Souths.	Moon. Dec.	Moon. Long. of Terminator.
		h m	h m	° ′	°
1	F	19 14	23 18	21 29 S	−82·1 E.
2	S	sets	……	22 54	+85·7 M.
3	**S**	4 13	0 16	22 58	+73·5 M.
4	M	5 19	1 15	21 35	+61·4 M.
5	Tu	6 35	2 14	18 50	+49·2 M.
6	W	7 56	3 10	14 56	+37·0 M.
7	Th	9 17	4 4	10 10	+24·8 M.
8	F	10 39	4 56	4 52 S	+12·7 M.
9	S	12 1	5 46	0 39 N	+ 0·6 M.
10	**S**	13 20	6 36	6 6	−11·6 M.
11	M	14 39	7 25	11 10	−23·8 M.
12	Tu	15 57	8 17	15 37	−36·0 M.
13	W	17 12	9 9	19 12	−48·1 M.
14	Th	18 21	10 3	21 42	−60·2 M.
15	F	19 24	10 58	22 58	−72·4 M.
16	S	rises	11 53	22 58	−84·5 M.
17	**S**	4 26	12 45	21 46	+83·3 E.
18	M	5 32	13 35	19 32	+71·1 E.
19	Tu	6 38	14 23	16 26	+59·0 E.
20	W	7 47	15 7	12 41	+46·8 E.
21	Th	8 54	15 50	8 30	+34·6 E.
22	F	9 59	16 31	4 1 N	+22·4 E.
23	S	11 5	17 12	0 36 S	+10·3 E.
24	**S**	12 12	17 53	5 14	− 1·8 E.
25	M	13 19	18 36	9 43	−14·0 E.
26	Tu	14 29	19 21	13 54	−26·2 E.
27	W	15 40	20 10	17 35	−38·4 E.
28	Th	16 52	21 3	20 32	−50·5 E.
29	F	18 0	21 59	22 28	−62·7 E.
30	S	19 0	22 58	23 7	−74·8 E.

Radiant-points of the principal Meteoric Showers of the Year.

By W. F. Denning.

Date.	Radiant.		Meteors.	Date.	Radiant.		Meteors.
	α °	*δ* °			*α* °	*δ*	
Jan. 2 ...	**230**	**+53**	Swift; long paths.	July 28 ...	**339**°	**−12**	Slow; long.
3 ...	156	+41	Swift.	30 ...	6	+35	Swift; streaks.
11 ...	220	+13	Swift; streaks.	Aug. 4 ...	30	+36	Swift; streaks.
17 ...	295	+53	Slow; bright.	4-10	350	+51	Swift.
22 ...	208	− 8	Very swift; streaks.	10 ...	**45**	**+57**	Swift; streaks.
25 ...	131	+32	Swift.	14 ...	311	+62	Rather swift.
29 ...	213	+52	Very swift.	15 ...	292	+53	Swift; bright.
Feb. 5-10	74	+43	Slow; bright.	16 ...	61	+48	Swift; streaks.
15 ...	236	+11	Swift; streaks.	22 ...	291	+60	Slow; bright.
15 ...	261	+ 4	Swift; streaks.	25 ...	5	+11	Slow; short.
20 ...	181	+34	Swift; bright.	Sept. 4 ...	346	± 0	Slow.
20 ...	263	+36	Swift; streaks.	5-15	62	+37	Swift; streaks.
Mar. 4 ...	175	+10	Slow; bright.	10 ...	73	+41	Swift; streaks.
14 ...	250	+54	Swift.	15 ...	48	+44	Very swift.
18 ...	316	+76	Slow; bright.	15 ...	77	+57	Swift; streaks.
24 ...	161	+58	Swift.	21 ...	31	+19	Slow.
27 ...	229	+32	Swift; small.	22 ...	335	+58	Slowish.
28 ...	263	+62	Rather swift.	27 ...	75	+15	Swift; streaks.
Apr. 12-24	210	−10	Slow fireballs.	Oct. 2 ...	230	+52	Slow; bright.
18 ...	231	+17	Swift; short.	4 ...	133	+79	Swift; streaks.
19 ...	218	+33	Slow; bright.	4 ...	310	+77	Slowish.
19 ..	229	− 2	Slow; long.	8 ...	77	+31	Swift; streaks.
20 ...	**270**	**+33**	Swift.	8-14	45	+58	Small; short.
25 ...	272	+21	Swift; short.	14 ...	133	+68	Rather swift.
30 ...	291	+59	Rather slow.	18 ...	**92**	**+15**	Swift; streaks.
May 1 ...	239	+46	Small; short.	20 ...	106	+12	Swift; streaks.
5 ...	254	−21	Slowish.	29 ...	109	+23	Very swift.
6 ...	**338**	**− 2**	Swift; streaks.	Nov. 1 ...	43	+22	Slow; bright.
7 ...	244	+ 7	Slow; bright.	2 ...	55	+ 9	Slow; bright.
11 ...	231	+27	Slow; small.	7 ...	102	+73	Very swift.
29 ...	264	+64	Slowish.	10-12	133	+31	Very swift; streal
30 ...	333	+27	Swift; streaks.	14-16	**150**	**+22**	Swift; streaks.
June 7 ...	249	−20	Slow; fireballs.	16-28	154	+41	Swift; streaks.
10 ...	261	+ 5	Very slow.	20 ...	62	+23	Slow; bright.
13 ...	310	+61	Swift; streaks.	23-27	**25**	**+44**	Very slow; train
15 ...	291	+52	Swift; small.	25-28	208	+43	Very swift.
15 ...	285	+23	Slowish.	30 ...	190	+58	Swift; streaks.
20 ...	335	+57	Swift.	Dec. 4 ...	162	+58	Swift; streaks.
26 ...	354	+39	Swift; streaks.	6 ...	80	+23	Slow; bright.
July 4 ...	303	+24	Swift.	8 ...	145	+ 7	Swift; streaks.
7 ...	333	+26	Swift.	8 ...	208	+71	Rather swift.
6-12	282	−13	Very slow.	10 ...	**108**	**+33**	Swift; short.
19 ...	314	+48	Swift; short.	12 ...	119	+29	Rather swift.
22 ...	16	+31	Swift; streaks.	22 ...	194	+67	Swift; streaks.
25 ...	48	+43	Swift; streaks.	25 ...	98	+31	Very slow.

The radiant-points of the more brilliant showers are indicated by heavier type. T Leonids of November will probably furnish a brilliant display in 1899.

The Perseids, with max. on August 10, are visible for a considerable period and their radia point exhibits an easterly motion amongst the stars. The following is an ephemeris:—

Date.	Radiant.		Date.	Radiant.		Date.	Radiant.	
	α °	°		*α* °	*δ* °		*α* °	*δ* °
July 19 ...	19	+51	July 29 ...	32	+54	Aug. 8 ...	42	+57
21 ...	22	+52	31 ...	34	+55	10 ...	45	+57
23 ...	25	+52	Aug. 2 ...	36	+55	12 ...	47	+57
25 ...	27	+53	4 ...	38	+56	14 ...	50	+58
27 ...	30	+54	6 ...	40	+56	16 ...	53	+58

1899.	Mercury (☿).						Venus (♀).						Mars (♂).					
	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m
Jan. 1...	17 24 21	20 13 S.	9·4	18 23	22 37	2 51	16 11 19	16 35 S.	44·2	16 50	21 26	2 2	8 34 16	22 48 N.	14·0	5 37	13 48	21 59
11...	17 49 22	21 48	6·6	18 23	22 27	2 30	16 32 40	17 9	37·6	16 35	21 8	1 41	8 19 54	23 55	14·4	4 35	12 54	21 13
21...	18 40 40	23 0	5·8	18 24	22 40	2 36	17 2 50	18 13	32·4	16 33	21 0	1 27	8 3 2	24 55	14·2	3 33	11 58	20 23
31...	19 42 1	22 30	5·2	19 2	23 2	3 2	17 39 21	19 16	28·2	16 36	20 57	1 18	7 47 0	25 36	13·8	2 33	11 3	19 33
Feb. 10...	20 47 38	19 51	5·0	19 12	23 29	3 46	18 20 25	19 55	25·0	16 42	20 59	1 16	7 34 42	25 54	13·0	1 38	10 11	18 44
20...	21 55 19	14 53	4·8	19 13	23 57	4 41	19 4 31	19 55	22·4	16 47	21 4	1 21	7 27 49	25 52	12·0	0 52	9 25	17 58
Mar. 2...	23 4 29	7 38 S.	5·0	18 59	0 24	5 49	19 50 22	19 7	20·4	16 50	21 11	1 32	7 26 38	25 35	11·0	0 14	8 45	17 16
12...	0 13 24	1 21 N.	5·6	18 56	0 54	6 52	20 36 55	17 28	18·6	16 47	21 18	1 49	7 30 34	25 8	10·0	23 42	8 10	16 38
22...	1 10 37	9 37	7·0	18 17	1 11	8 5	21 23 18	14 59	17·2	16 40	21 25	2 10	7 38 49	24 32	9·2	23 15	7 39	16 3
Apr. 1...	1 34 41	13 22	9·2	17 43	0 56	8 9	22 9 2	11 47	16·0	16 28	21 31	2 34	7 50 27	23 46	8·4	22 52	7 11	15 30
11...	1 20 56	11 3	11·4	17 2	0 3 23 57	7 4	22 54 0	8 1	15·0	16 13	21 36	2 59	8 4 42	22 51	7·8	22 34	6 46	14 58
21...	1 1 27	6 13	11·2	16 24	23 0	5 36	23 38 18	3 49 S.	14·2	15 56	21 41	3 26	8 20 58	21 47	7·2	22 19	6 23	14 27
May 1...	1 6 23	4 26	9·6	16 2	22 28	4 54	0 22 17	0 38 N.	13·4	15 39	21 46	3 53	8 38 42	20 32	6·6	22 5	6 1	13 57
11...	1 35 35	6 32	8·0	15 42	22 19	4 56	1 6 27	5 8	12·8	15 22	21 51	4 20	8 57 33	19 6	6·2	21 54	5 41	13 28
21...	2 22 47	11 14	6·6	15 28	22 29	5 30	1 51 18	9 32	12·2	15 4	21 56	4 48	9 17 13	17 29	6·0	21 48	5 21	12 58
31...	3 27 2	17 16	5·6	15 19	22 55	6 31	2 37 22	13 37	11·8	14 48	22 3	5 18	9 37 28	15 43	5·6	21 36	5 2	12 28
June 10...	4 50 37	22 46	5·2	15 28	23 41	7 54	3 25 3	17 12	11·4	14 35	22 11	5 47	9 58 10	13 46	5·4	21 28	4 43	11 58
20...	6 25 41	24 57	5·2	16 2	0 32	9 2	4 14 34	20 6	11·0	14 27	22 22	6 17	10 19 13	11 41	5·2	21 22	4 25	11 28
30...	7 51 29	22 53	5·6	17 3	1 18	9 33	5 5 48	22 7	10·6	14 26	22 34	6 42	10 40 33	9 27	5·0	21 19	4 7	10 58
July 10...	8 58 19	18 15	6·4	18 4	1 46	9 28	5 58 21	23 7	10·4	14 32	22 47	7 2	11 2 9	7 6	4·8	21 10	3 49	10 28
20...	9 46 0	12 52	7·6	18 44	1 54	9 4	6 51 26	23 1	10·2	14 46	23 0	7 14	11 24 3	4 39	4·6	21 7	3 32	9 57
30...	10 13 5	8 15	9·0	18 55	1 41	8 27	7 44 10	21 48	10·0	15 6	23 14	7 22	11 46 16	2 8 N.	4·6	21 1	3 14	9 27
Aug. 9...	10 13 25	6 16	10·4	18 28	1 2	7 36	8 35 50	19 31	10·0	15 34	23 26	7 18	12 8 51	0 27 S.	4·4	20 58	2 58	8 58
19...	9 47 4	8 40	10·8	17 4	23 50	6 36	9 25 55	16 18	9·8	16 6	23 36	7 6	12 31 54	3 4	4·4	20 53	2 41	8 29
29...	9 30 49	12 46	8·8	15 49	22 58	6 7	10 14 19	12 20	9·8	16 37	23 45	6 53	12 55 29	5 41	4·2	20 51	2 25	8 0
Sept. 8...	10 2 24	12 52	6·6	15 45	22 55	6 5	11 1 15	7 48	9·8	17 9	23 53	6 37	13 19 42	8 17	4·2	20 49	2 10	7 31
18...	11 6 7	7 45	5·4	16 36	23 20	6 4	11 47 11	2 54 N.	9·8	17 42	23 59	6 17	13 44 40	10 50	4·2	20 49	1 56	7 3
28...	12 13 16	0 11 N.	4·8	17 42	23 48	5 54	12 32 45	2 9 S.	9·8	18 12	0 5	5 58	14 10 28	13 17	4·0	20 48	1 42	6 36
Oct. 8...	13 15 53	7 27 S.	4·8	18 41	0 8	5 35	13 18 38	7 10	9·8	18 44	0 11	5 38	14 37 13	15 35	4·0	20 47	1 29	6 11
18...	14 15 33	14 14	4·8	19 39	0 29	5 19	14 5 30	11 56	10·0	19 16	0 18	5 20	15 4 58	17 44	4·0	20 49	1 18	5 47
28...	15 14 22	19 45	5·0	20 30	0 48	5 6	14 53 57	16 14	10·0	19 48	0 27	5 6	15 33 47	19 38	4·0	20 49	1 7	5 25
Nov. 7...	16 12 17	23 38	5·6	21 14	1 6	4 58	15 44 22	19 50	10·2	20 21	0 38	4 55	16 3 40	21 17	4·0	20 51	0 58	5 5
17...	17 3 22	25 24	6·6	21 39	1 18	4 59	16 36 47	22 31	10·4	20 51	0 51	4 51	16 34 35	22 36	4·0	20 50	0 49	4 48
27...	17 24 44	24 29	8·6	21 16	1 0	4 44	17 30 49	24 7	10·6	21 18	1 6	4 54	17 6 25	23 34	4·0	20 50	0 42	4 34
Dec. 7...	16 43 43	20 28	9·8	19 19	23 30	3 41	18 25 40	24 30	10·8	21 36	1 22	5 8	17 39 1	24 8	4·0	20 47	0 35	4 23
17...	16 18 29	18 32	8·0	18 7	22 32	2 57	19 20 14	23 39	11·0	21 45	1 37	5 29	18 12 10	24 15	4·0	20 42	0 29	4 16
27...	16 49 40	20 42 S.	6·4	18 16	22 27	2 38	20 13 30	21 36 S.	11·4	21 46	1 51	5 56	18 45 36	23 56 S.	4·0	20 33	0 23	4 13

Mercury.

	d h	d h	d h	d h
Gr. elong. E....	Mar. 24 11;	July 21 23;	Nov. 16 4	
„ W....	Jan. 11 14;	May 9 16;	Sept. 4 19;	Dec. 25 1
Sup. conj.	Feb. 27 3;	June 14 7;	Sept. 30 16;	
Inf. conj.	Apr. 11 20;	Aug. 18 21;	Dec. 5 17	
Stationary ...	Apr. 1 17;	Apr. 24 5;	Aug. 4 4;	Aug. 27 21

Venus.

	d h
Gr. elong. W...	Feb. 10 19
Sup. conj.	Sept. 15 20
Gr. brilliancy..	Jan. 5 19

Superior Planets.

	Conjunction. d h	Opposition. d h	Quadrature. d h	d h	Stationary. d h	d h
Mars ...	......	Jan. 18 12	Apr. 23 6	......	Feb. 27 3	
Jupiter...	Nov. 12 20	Apr. 25 7	Jan. 28 19;	July 24 1	Feb. 23 21;	June 27 16
Saturn...	Dec. 17 17	June 11 2	Mar. 13 16;	Sept. 9 20	Apr. 2 0;	Aug. 21 9
Uranus...	Nov. 30 4	May 27 2	Feb. 26 6;	Aug. 27 3	Mar. 12 14;	Aug. 12 10

Jupiter (♃). Saturn (♄).

1899.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	S
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h
Jan. 1	14 16 34	12 26 S.	31·4	14 32	19 30	0 28	17 7 13	21 30 S.	14·0	18 15	22 20	2
21	14 26 7	13 11	33·2	13 27	18 21	23 15	17 16 17	21 41	14·2	17 7	21 11	1
Feb. 10	14 31 44	13 34	35·4	12 15	17 8	22 1	17 23 53	21 47	14·6	15 56	19 59	c
Mar. 2	14 32 41	13 34	37·6	10 57	15 50	20 43	17 29 24	21 50	15·0	14 45	18 46	22
22	14 28 49	13 12	39·6	9 33	14 27	19 21	17 32 21	21 51	15·6	13 27	17 30	21
Apr. 11	14 21 2	12 32	40·8	8 3	13 1	17 59	17 32 30	21 49	16·2	12 8	16 12	2c
May 1	14 11 20	11 43	41·2	6 30	11 32	16 34	17 29 55	21 47	16·6	10 48	14 51	18
21	14 2 22	10 58	40·4	4 59	10 5	15 11	17 25 6	21 42	16·8	9 23	13 27	17
June 10	13 56 22	10 30	38·8	3 31	8 40	13 49	17 19 1	21 38	17·0	7 58	12 2	16
30	13 54 38	10 27	36·8	2 11	7 20	12 29	17 12 51	21 33	16·8	6 33	10 38	14
July 20	13 57 25	10 48	34·6	0 57	6 4	11 11	17 7 49	21 30	16·6	5 9	9 14	13
Aug. 9	14 4 20	11 30	32·8	23 49	4 53	9 57	17 4 53	21 30	16·2	3 47	7 52	11
29	14 14 46	12 29	31·2	22 45	3 44	8 43	17 4 33	21 33	15·6	2 29	6 34	1c
Sept. 18	14 28 1	13 39	30·0	21 47	2 39	7 31	17 6 59	21 40	15·2	1 13	5 17	9
Oct. 8	14 43 23	14 55	29·0	20 51	1 36	6 21	17 11 57	21 50	14·8	0 1	4 4	8
28	15 0 15	16 11	28·6	19 56	0 34	5 12	17 19 6	22 1	14·4	22 50	2 52	6
Nov. 17	15 17 57	17 24	28·6	18 59	23 30	4 1	17 27 52	22 14	14·0	21 45	1 42	5
Dec. 7	15 35 48	18 29	28·8	18 5	22 29	2 53	17 37 40	22 19	14·0	20 32	0 33	4
27	15 53 1	19 25 S.	29·6	17 8	21 27	1 46	17 47 51	22 25 S.	14·0	19 22	23 21	3

Uranus (♅). Neptune (♆).

Jan. 1	16 16 23	21 12 S.	3·5	17 22	21 29	1 36	5 29 24	21 55 N.	2·7	6 41	10 44	14
31	16 22 4	21 25	3·5	15 30	19 37	23 42	5 26 32	21 54	2·7	4 40	8 43	12
Mar. 2	16 24 56	21 32	3·6	13 37	17 42	21 47	5 25 24	21 54	2·6	2 41	6 44	1c
Apr. 1	16 24 27	21 31	3·6	11 38	15 43	19 48	5 26 31	21 57	2·6	0 44	4 47	8
May 1	16 21 2	21 24	3·7	9 36	13 42	17 48	5 29 25	22 0	2·6	22 50	2 52	6
31	16 15 59	21 12	3·7	7 32	11 39	15 46	5 33 44	22 4	2·5	20 57	0 59	5
June 30	16 11 12	21 0	3·6	5 28	9 36	13 44	5 38 31	22 7	2·5	18 59	23 1	3
July 30	16 8 23	20 56	3·6	3 27	7 36	11 45	5 42 53	22 9	2·5	17 7	21 8	1
Aug. 29	16 8 33	20 54	3·6	1 29	5 38	9 47	5 45 58	22 9	2·6	15 12	19 13	23
Sept. 28	16 12 54	21 3	3·5	23 35	3 43	7 51	5 47 10	22 8	2·6	13 15	17 16	21
Oct. 28	16 17 50	21 18	3·5	21 45	1 51	5 57	5 46 14	22 7	2·6	11 15	15 17	19
Nov. 27	16 25 19	21 36	3·4	19 54	0 1 23 57	4 4	5 43 30	22 6	2·7	9 14	13 16	17
Dec. 27	16 33 0	21 53 S.	3·4	18 4	22 7	2 10	5 39 54	22 4 N.	2·7	7 13	11 15	15

Ceres. Pallas.

1899.	R.A. Noon.	Dec. Noon.	Transits.	1899.	R.A. Noon.	Dec. Noon.	Transits.
	h m	° ′	h m		h m	° ′	h m
Apr. 6 ...	14 39	1 59 S.	13 38	Mar. 13 ...	13 51	8 44 N.	14 25
18 ...	14 20	1 25	12 42	25 ...	13 45	12 58	13 32
30 ...	14 19	1 5	11 44	April 6 ...	13 37	16 50	12 36
May 12 ...	14 9	1 4 S.	10 47	18 ...	13 28	19 56	11 40
				30 ...	13 20	22 3 N.	10 45

Juno. Vesta.

May 24 ...	17 47	5 7 S.	13 37	Sept. 21 ...	1 56	0 20 S.	13 53
June 5 ...	17 38	4 36	12 41	Oct. 3 ...	1 47	1 33	12 56
17 ...	17 28	4 23	11 44	15 ...	1 36	2 40	11 58
29 ...	17 18	4 28 S.	10 46	27 ...	1 24	3 28	11 0
				Nov. 8 ...	1 15	3 48 S.	10 3

The times of rising and setting correspond to the adjacent transit, irrespective of the day given i the first column.

ECLIPSES IN 1899.

In the year 1899 there will be three Eclipses of the Sun and two of the Moon.

I.—*A Partial Eclipse of the Sun*, 1899, *January* 11.

Invisible at Greenwich. Greatest Eclipse Jan. 11^{d} 10^{h} 38^{m}·0. Magnitude (Sun's diam.=1), 0·718. This Eclipse will be visible in Kamskatka, Alaska, and the North Pacific.

II.—*A Partial Eclipse of the Sun*, 1899, *June* 7.

Visible at Greenwich and throughout Northern Europe and Northern Asia. Greatest Eclipse June 7^{d} 18^{h} 34^{m}·0. Magnitude (Sun's diam.=1), 0·611; in longitude 99° 5′ W., latitude 67° 18′ N.

Details for British Stations. The times are Greenwich Mean times, except for Dublin, where local mean time is used:—

Station.	Begins.	Greatest Eclipse.	Ends.	Magnitude.
	h m	h m	h m	
Greenwich	16 42·8	17 17·4	17 53·4	0·188
Cambridge	16 43·2	17 18·6	17 55·5	0·197
Oxford	16 42·8	17 18·2	17 55·1	0·200
Liverpool	16 43·8	17 21·5	18 0·9	0·233
Edinburgh	16 45·7	17 25·9	18 7·8	0·263
Dublin	16 18·6	16 57·3	17 37·6	0·253

At Greenwich (and approximately throughout the British Isles) first contact is 42° from N. point towards W., and last contact 29° from N. towards E. (direct image).

III.—*A Total Eclipse of the Moon*, 1899, *June* 22–23.

Invisible at Greenwich.

Greenwich M.T. of ☍ in R.A.: June 23^{d} 2^{h} 20^{m} 28^{s}·8.

	d h m		d h m
First contact with Penumbra	22 23 37·5	With shadow	23 0 32·8
Beginning of Total Phase	23 1 32·8	End	3 3·0
Second contact with Penumbra	4 58·3	With shadow	4 3·0

Magnitude of the Eclipse (Moon's diameter=1), 1·487.

First contact with shadow 96° from N. point towards E.; last contact 109° towards W.

The Eclipse is visible in India, China, Australia, New Zealand, and the Pacific Ocean.

IV.—*An Annular Eclipse of the Sun*, 1899, *Dec.* 2.

Invisible at Greenwich. The central line passes over the Antarctic Circle.

A small partial eclipse is visible in S.W. Australia, Tasmania, and S. New Zealand.

V.—*A Partial (almost Total) Eclipse of the Moon*, 1899, *Dec.* 16.

Visible at Greenwich.

Greenwich M.T. of ☍ in R.A.: Dec. 16^{d} 13^{h} 29^{m} 4^{s}·7.

☾'s R.A. 5^{h} 37^{m} 55^{s}·01. Hourly motion 139^{s}·17. ☉'s 11^{s}·08.

	° ′ ″			′ ″	
☾'s Declination	22 55 37·3	N.	Hourly motion	1 43·2	S.
☉'s Declination	23 21 16·4	S.	Hourly motion	5·9	S.
☾'s Equat. Hor. Parallax	56 10·63		True Semidiameter..	15 18·61	
☉'s Equat. Hor. Parallax	8·99		True Semidiameter..	16 15·38	

	h m		h m
First contact with Penumbra	10 33·7	With shadow	11 44·6
Second ,, ,, ,,	16 18·1	,, ,,	15 7·2

Magnitude of the Eclipse (Moon's diam.=1), 0·995.

First contact with the shadow at 66° from N. point towards E.; last contact at 59° towards W.

The Eclipse is visible in Western Asia, throughout Europe and Africa, and in Eastern America.

Date.	Star.	Mag.	Disap. M.T.	Disap. P.	Reap. M.T.	Reap. P.
Jan.			h m	°	h m	°
3	D.M. $-6°$ 3518	6·4	18 50	111	20 4	308
5	D.M. $-15°$ 3715	6·5	13 33†	139	14 29	273
7	42 Libræ	5·2	17 3	60	17 48	335
19	μ Arietis.........	5·8	8 5	45	9 14	282
22	121 Tauri	5·4	15 33	99	16 29	279
24	56 Geminorum .	5·0	17 32	108	18 26	286
29	p^2 Leonis	6·2	12 22	185	12 57	240
30	B.A.C. 4006 ...	5·7	13 39	126	14 57	302
Feb.						
1	D.M. $-13°$ 3692	6·5	11 35*	68	12 16	346
12	19 Piscium......	5·2	7 35	78	8 28‡	235
18	103 Tauri	5·5	8 53	96	10 11	273
19	3 Geminorum ..	6·5	13 57	152	14 34	232
22	B.A.C. 2888 ...	6·5	12 42	134	13 51	280
25	36 Sextantis ...	6·5	7 22	133	8 25	280
25	55 Leonis	6·0	15 20	164	16 11	256
Mar.						
3	B.A.C. 5254 ...	5·4	13 5†	74	14 2	318
20	56 Geminorum .	5·0	5 32	124	6 50	265
20	61 Geminorum .	5·7	8 50	83	9 59	320
25	e Leonis	5·1	16 52	90	17 45†	316
26	D.M. $-6°$ 3518 .	6·4	13 57	202	14 8	218
27	D.M. $-10°$ 3570	6·0	10 0	81	10 56	340
28	83 Virginis......	5·8	9 57	77	10 49	339
29	B.A.C. 4867 ...	6·4	15 13	148	16 6	244
29	B.A.C. 4879 ...	6·3	16 12	184	16 28	208
30	D.M. $-22°$ 3989	6·0	11 59	53	12 40	343
Apr.						
4	B.A.C. 7145 ...	6·0	14 52†	349	14 58†	340
15	μ Geminorum ..	3·2	11 26	65	12 8	319
17	3 Cancri	6·0	10 44	182	11 8	224
19	h Leonis	5·4	9 10	154	10 14	267
21	p^2 Leonis	6·2	10 1	105	11 14	319
22	B.A.C. 4006 ...	5·7	11 12	95	12 20	325
24	D.M. $-13°$ 3692	6·5	7 9	127	8 14	288
25	D.M. $-17°$ 4053	6·5	8 19	64	9 1	346
25	B.A.C. 4739 ...	6·2	9 17	49	9 45	1
26	B.A.C. 5023 ...	5·8	9 53	122	11 1	277
28	θ Ophiuchi ...	3·4	11 56	89	13 6	280
30	B.A.C. 6671 ...	6·5	15 25	138	15 57	189
May	[(2nd *)					
23	B.A.C. 4923	6·3	11 58	59	12 51	330
24	B.A.C. 5254 ...	5·4	10 15	125	11 25	261
25	B.A.C. 5709 ...	6·3	11 59	106	13 12	259
25	26 Ophiuchi ...	6·1	12 4	83	13 18	281
26	7 Sagittarii ...	5·4	11 39	51	12 39	302
26	9 Sagittarii ...	5·7	12 3	72	13 15	278
26	B.A.C. 6111 ...	6·5	12 41	85	13 56	263
une						
1	19 Piscium......	5·2	14 53	20	15 40	284
18	D.M. $-15°$ 3715	6·5	12 7	125	13 6†	267
19	B.A.C. 4814 ...	6·4	12 57‡	139	13 47†	242
23	B.A.C. 6343 ...	5·8	10 34	102	11 41	240
24	f Sagittarii ...	5·1	13 17	30	14 12	290
25	B.A.C. 7145 ...	6·0	9 50	92	10 50	232
26	D.M. $-11°$ 5640	6·3	10 45	65	11 48	250
27	κ Aquarii	5·5	12 59	40	14 2	262
28	κ Piscium	5·0	11 22	32	12 10	278

Date.	Star.	Mag.	Disap. M.T.	Disap. P.
July			h m	°
3	D.M. $+21°$ 539 .	5·7	12 40†	83
3	32 Tauri	5·7	14 57	39
19	B.A.C. 5709 ...	6·3	8 6	113
19	26 Ophiuchi ...	6·1	8 9	90
20	7 Sagittarii ...	5·4	7 39	58
20	9 Sagittarii ...	5·7	8 4	77
20	B.A.C. 6111 ...	6·5	8 41	89
23	D.M. $-13°$ 5891	6·5	10 59	148
24	B.A.C. 7717 ...	6·5	8 53	63
31	D.M. $+23°$ 715 .	6·5	14 6	24
Aug.				
14	D.M. $-22°$ 3989	6·0	8 25	94
17	B.A.C. 6343 ...	5·8	7 36	134
18	f Sagittarii	5·1	10 28	50
18	57 Sagittarii ...	6·2	13 43	13
19	B.A.C. 7145 ...	6·0	6 49	87
20	D.M. $-11°$ 5640	6·3	7 2	46
22	16 Piscium......	5·6	9 21	33
22	19 Piscium......	5·2	15 1	48
26	τ^2 Arietis	5·2	12 5	91
26	65 Arietis	5·6	12 55	106
27	B.A.C. 1289 ...	6·2	9 55	69
27	v^1 Tauri	4·6	16 20	122
27	v^2 Tauri	5·5	16 49	87
29	η Geminorum...	Var.	16 9	92
30	ζ Geminorum...	Var.	14 59	120
Sept.				
12	39 Ophiuchi ...	6·0	8 47	28
13	1 Sagittarii......	5·3	5 5	51
17	B.A.C. 7717 ...	6·5	6 18	64
23	A^1 Tauri.........	4·5	15 29	125
Oct.				
10	4 Sagittarii......	4·6	7 3	96
12	57 Sagittarii ...	6·2	4 19	58
16	16 Piscium......	5·6	6 3	41
16	19 Piscium......	5·2	11 53	52
19	27 Arietis	6·5	9 22	78
21	κ^1 Tauri	4·6	8 37	76
21	κ^2 Tauri	5·5	8 38	97
Nov.				
12	κ Piscium	5·0	11 11	136
17	A^1 Tauri.........	4·5	10 21	54
17	A^2 Tauri.........	6·5	10 36	76
17	B.A.C. 1289 ...	6·2	15 10	23
19	Neptune	...	6 10	95
19	B.A.C. 1970 ...	6·5	16 20	29
21	B.A.C. 2649 ...	6·3	19 10	183
25	55 Leonis	6·0	14 11	108
Dec.				
14	D.M. $+20°$ 602 .	6·2	7 43	148
15	κ^2 Tauri	5·5	2 48	57
15	κ^1 Tauri	4·6	2 55	31
16	Neptune	...	15 36	158
18	f Geminorum...	5·2	18 31	161
19	D.M. $+16°$ 1679	6·5	11 0	92
25	D.M. $-10°$ 3570	6·0	14 39	128
26	83 Virginis	5·8	16 22	172

* Star rising. ‡ Star setting. † Star below horizon.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i. e.* from *bottom of the* Moon's inverted image towards the right.

The following "near approaches" are also given in the 'Nautical Almanac,' and are especially interesting to observers rather South and rather North of Greenwich respectively:—

North Near Approaches.

Date.	Star.	Mag.	G.M.T.	Angle.	Date.	Star.	Mag.	G.M.T.	Angle.
Jan.			h m	°	July			h m	°
6	B.A.C. 4867 ...	6·4	20 20	18	3	B.A.C. 1189 ...	6·0	12 31	349
Feb.					10	*h* Leonis.........	5·4	8 35	23
7	B.A.C. 6671 ...	6·5	18 30	350	Aug.				
Mar.					2	μ Geminorum .	3·2	14 14	0
2	B.A.C. 4923	6·3	13 39	22	21	κ Aquarii	5·5	8 19	335
	(2nd *).				Oct.				
2	D.M. − 21°4030	6·5	19 7	11	14	c^2 Capricorni...	6·2	6 1	334
6	B.A.C. 6485 ...	6·5	15 49	354	20	65 Arietis	5·6	7 56	344
17	99 Tauri	6·0	12 57	4	21	τ Tauri	4·4	18 2	2
Apr.					Nov.				
5	D.M. − 11°5640	6·3	16 45	338	9	τ^2 Capricorni...	5·3	7 49	334
23	*q* Virginis	5·7	8 9	31	16	τ^2 Arietis	5·2	18 52	355
24	75 Virginis ...	5·7	14 15	20	16	65 Arietis	5·6	19 31	355
May					Dec.				
14	*g* Geminorum ..	5·1	11 29	17	23	*e* Leonis.........	5·1	19 36	29
19	*e* Leonis.........	5·1	9 28	31					
June									
18	75 Virginis ...	5·7	7 58	26					

South Near Approaches.

Jan.			h m	°	Aug.			h m	°
26	B.A.C. 2888 ...	6·5	4 54	193	29	B.A.C. 1970 ...	6·5	13 59	179
Feb.					Sept.				
19	1 Geminorum ..	4·3	11 26	192	23	A^2 Tauri	6·5	16 17	172
Mar.					Oct.				
3	B.A.C. 5253 ...	5·6	13 49	197	20	D.M. + 20°602..	6·2	15 16	171
16	32 Tauri	5·7	10 15	178	21	56 Tauri.........	5·4	6 42	170
30	42 Libræ	5·2	14 1	196	25	5 Cancri.........	6·4	10 6	189
Apr.					Nov.				
2	26 Sagittarii ...	6·2	13 56	177	17	56 Tauri.........	5·4	18 30	181
13	v^2 Tauri.........	5·5	8 35	182	21	*f* Geminorum ..	5·2	8 3	188
15	η Geminorum ..	Var.	8 6	194	Dec.				
May					19	29 Cancri	5·9	18 53	204
27	B.A.C. 6485 ...	6·5	10 10	174	24	D.M. − 6·3518 ..	6·4	16 56	211

TABLE OF BESSEL'S MEAN REFRACTIONS.

Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.
°	′ ″	°	′ ″	°	′ ″	°	′ ″
0	0·0	56	1 25·4	68	2 21·9	78	4 25·0
10	10·2	58	1 32·1	69	2 29·3	79	4 48·5
20	21·0	60	1 39·7	70	2 37·3	80	5 16·2
30	33·3	61	1 43·8	71	2 46·1	81	5 49·3
35	40·4	62	1 48·2	72	2 55·8	82	6 29·6
40	48·4	63	1 52·8	73	3 6·6	83	7 19·7
45	57·7	64	1 57·8	74	3 18·6	84	8 23·3
50	1 8·7	65	2 3·2	75	3 32·1	85	9 46·5
52	1 13·8	66	2 8·9	76	3 47·4		
54	1 19·3	67	2 15·2	77	4 4·9		

Radiant-points of the principal Meteoric Showers of the Year.

By W. F. Denning.

Date.	Radiant.		Meteors.	Date.	Radiant.		Meteors.
	α °	δ °			α °	δ	
Jan. 2 ...	**230**	**+53**	Swift; long paths.	July 28 ...	**339**	**−12**	Slow; long.
3 ...	156	+41	Swift.	30 ...	6	+35	Swift; streaks.
11 ...	220	+13	Swift; streaks.	Aug. 4 ...	30	+36	Swift; streaks.
17 ...	295	+53	Slow; bright.	4-10	350	+51	Swift.
22 ...	208	− 8	Very swift; streaks.	10 ...	**45**	**+57**	Swift; streaks.
25 ...	131	+32	Swift.	14 ...	311	+62	Rather swift.
29 ...	213	+52	Very swift.	15 ...	292	+53	Swift; bright.
Feb. 5-10	74	+43	Slow; bright.	16 ...	61	+48	Swift; streaks.
15 ...	236	+11	Swift; streaks.	22 ...	291	+60	Slow; bright.
15 ...	261	+ 4	Swift; streaks.	25 ...	5	+11	Slow; short.
20 ...	181	+34	Swift; bright.	Sept. 4 ...	346	± 0	Slow.
20 ...	263	+36	Swift; streaks.	5-15	62	+37	Swift; streaks.
Mar. 4 ...	175	+10	Slow; bright.	10 ...	73	+41	Swift; streaks.
14 ...	250	+54	Swift.	15 ...	48	+44	Very swift.
18 ...	316	+76	Slow; bright.	15 ...	77	+57	Swift; streaks.
24 ...	161	+58	Swift.	21 ...	31	+19	Slow.
27 ...	229	+32	Swift; small.	22 ...	335	+58	Slowish.
28 ...	263	+62	Rather swift.	27 ...	75	+15	Swift; streaks.
Apr. 12-24	210	−10	Slow fireballs.	Oct. 2 ...	230	+52	Slow; bright.
18 ...	231	+17	Swift; short.	4 ...	133	+79	Swift; streaks.
19 ...	218	+33	Slow; bright.	4 ...	310	+77	Slowish.
19 ..	229	− 2	Slow; long.	8 ...	77	+31	Swift; streaks.
20 ...	**270**	**+33**	Swift.	8-14	45	+58	Small; short.
25 ...	272	+21	Swift; short.	14 ...	133	+68	Rather swift.
30 ...	291	+59	Rather slow.	18 ...	**92**	**+15**	Swift; streaks.
May 1 ...	239	+46	Small; short.	20 ...	106	+12	Swift; streaks.
5 ...	254	−21	Slowish.	29 ...	109	+23	Very swift.
6 ...	**338**	**− 2**	Swift; streaks.	Nov. 1 ...	43	+22	Slow; bright.
7 ...	244	+ 7	Slow; bright.	2 ...	55	+ 9	Slow; bright.
11 ...	231	+27	Slow; small.	7 ...	102	+73	Very swift.
29 ...	264	+64	Slowish.	10-12	133	+31	Very swift; streaks.
30 ...	333	+27	Swift; streaks.	14-16	**150**	**+22**	Swift; streaks.
June 7 ...	249	−20	Slow; fireballs.	16-28	154	+41	Swift; streaks.
10 ...	261	+ 5	Very slow.	20 ...	62	+23	Slow; bright.
13 ...	310	+61	Swift; streaks.	23-27	**25**	**+44**	Very slow; trains.
15 ...	291	+52	Swift; small.	25-28	208	+43	Very swift.
15 ...	285	+23	Slowish.	30 ...	190	+58	Swift; streaks.
20 ...	335	+57	Swift.	Dec. 4 ...	162	+58	Swift; streaks.
26 ...	354	+39	Swift; streaks.	6 ...	80	+23	Slow; bright.
July 4 ...	303	+24	Swift.	8 ...	145	+ 7	Swift; streaks.
7 ...	333	+26	Swift.	8 ...	208	+71	Rather swift.
6-12	282	−13	Very slow.	10 ...	**108**	**+33**	Swift; short.
19 ...	314	+48	Swift; short.	12 ...	119	+29	Rather swift.
22 ...	16	+31	Swift; streaks.	22 ...	194	+67	Swift; streaks.
25 ...	48	+43	Swift; streaks.	25 ...	98	+31	Very slow.

The radiant-points of the more brilliant showers are indicated by heavier type. The Leonids of November will probably furnish a brilliant display in 1899.

The Perseids, with max. on August 10, are visible for a considerable period and their radiant-point exhibits an easterly motion amongst the stars. The following is an ephemeris:—

Date.	Radiant.		Date.	Radiant.		Date.	Radiant.	
	α °	°		α °	δ °		α °	δ °
July 19 ...	19	+51	July 29 ...	32	+54	Aug. 8 ...	42	+57
21 ...	22	+52	31 ...	34	+55	10 ...	45	+57
23 ...	25	+52	Aug. 2 ...	36	+55	12 ...	47	+57
25 ...	27	+53	4 ...	38	+56	14 ...	50	+58
27 ...	30	+54	6 ...	40	+56	16 ...	53	+58

	Mercury (☿).						Venus (♀).						Mars (♂).					
1899.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m
Jan. 1...	17 24 21	20 13 S.	9·4	18 23	22 37	2 51	16 11 19	16 35 S.	44·2	16 50	21 26	2 2	8 34 16	22 48 N.	14·0	5 37	13 48	21 59
11...	17 49 22	21 48	6·6	18 23	22 27	2 30	16 32 40	17 9	37·6	16 35	21 8	1 41	8 19 54	23 55	14·4	4 35	12 54	21 13
21...	18 40 40	23 0	5·8	18 24	22 40	2 36	17 2 50	18 13	32·4	16 33	21 0	1 27	8 3 2	24 55	14·2	3 33	11 58	20 23
31...	19 42 1	22 30	5·2	19 2	23 2	3 2	17 39 21	19 16	28·2	16 36	20 57	1 18	7 47 0	25 36	13·8	2 33	11 3	19 33
Feb. 10...	20 47 38	19 51	5·0	19 12	23 29	3 46	18 20 25	19 55	25·0	16 42	20 59	1 16	7 34 42	25 54	13·0	1 38	10 11	18 44
20...	21 55 19	14 53	4·8	19 13	23 57	4 41	19 4 31	19 55	22·4	16 47	21 4	1 21	7 27 49	25 52	12·0	0 52	9 25	17 58
Mar. 2...	23 4 29	7 38 S.	5·0	18 59	0 24	5 49	19 50 22	19 7	20·4	16 50	21 11	1 32	7 26 38	25 35	11·0	0 14	8 45	17 16
12...	0 13 24	1 21 N.	5·6	18 56	0 54	6 52	20 36 55	17 28	18·6	16 47	21 18	1 49	7 30 34	25 8	10·0	23 42	8 10	16 38
22...	1 10 37	9 37	7·0	18 17	1 11	8 5	21 23 18	14 59	17·2	16 40	21 25	2 10	7 38 49	24 32	9·2	23 15	7 39	16 3
Apr. 1...	1 34 41	13 22	9·2	17 43	0 56	8 9	22 9 2	11 47	16·0	16 28	21 31	2 34	7 50 27	23 46	8·4	22 52	7 11	15 30
11...	1 20 56	11 3	11·4	17 2	0 3 23 57	7 4	22 54 0	8 1	15·0	16 13	21 36	2 59	8 4 42	22 51	7·8	22 34	6 46	14 58
21...	1 1 27	6 13	11·2	16 24	23 0	5 36	23 38 18	3 49 S.	14·2	15 56	21 41	3 26	8 20 58	21 47	7·2	22 19	6 23	14 27
May 1...	1 6 23	4 26	9·6	16 2	22 28	4 54	0 22 17	0 38 N.	13·4	15 39	21 46	3 53	8 38 42	20 32	6·6	22 5	6 1	13 57
11...	1 35 35	6 32	8·0	15 42	22 19	4 56	1 6 27	5 8	12·8	15 22	21 51	4 20	8 57 33	19 6	6·2	21 54	5 41	13 28
21...	2 22 47	11 14	6·6	15 28	22 29	5 30	1 51 18	9 32	12·2	15 4	21 56	4 48	9 17 13	17 29	6·0	21 48	5 21	12 58
31...	3 27 2	17 16	5·6	15 19	22 55	6 31	2 37 22	13 37	11·8	14 48	22 3	5 18	9 37 28	15 43	5·6	21 36	5 2	12 28
June 10...	4 50 37	22 46	5·2	15 28	23 41	7 54	3 25 3	17 12	11·4	14 35	22 11	5 47	9 58 10	13 46	5·4	21 28	4 43	11 58
20...	6 25 41	24 57	5·2	16 2	0 32	9 2	4 14 34	20 6	11·0	14 27	22 22	6 17	10 19 13	11 41	5·2	21 22	4 25	11 28
30...	7 51 29	22 53	5·6	17 3	1 18	9 33	5 5 48	22 7	10·6	14 26	22 34	6 42	10 40 33	9 27	5·0	21 19	4 7	10 58
July 10...	8 58 19	18 15	6·4	18 4	1 46	9 28	5 58 21	23 7	10·4	14 32	22 47	7 2	11 2 9	7 6	4·8	21 10	3 49	10 28
20...	9 46 0	12 52	7·6	18 44	1 54	9 4	6 51 26	23 1	10·2	14 46	23 0	7 14	11 24 3	4 39	4·6	21 7	3 32	9 57
30...	10 13 5	8 15	9·0	18 55	1 41	8 27	7 44 10	21 48	10·0	15 6	23 14	7 22	11 46 16	2 8 N.	4·6	21 1	3 14	9 27
Aug. 9...	10 13 25	6 16	10·4	18 28	1 2	7 36	8 35 50	19 31	10·0	15 34	23 26	7 18	12 8 51	0 27 S.	4·4	20 58	2 58	8 58
19...	9 47 4	8 40	10·8	17 4	23 50	6 36	9 25 55	16 18	9·8	16 6	23 36	7 6	12 31 54	3 4	4·4	20 53	2 41	8 29
29...	9 30 49	12 46	8·8	15 49	22 58	6 7	10 14 19	12 20	9·8	16 37	23 45	6 53	12 55 29	5 41	4·2	20 51	2 25	8 0
Sept. 8...	10 2 24	12 52	6·6	15 45	22 55	6 5	11 1 15	7 48	9·8	17 9	23 53	6 37	13 19 42	8 17	4·2	20 49	2 10	7 31
18...	11 6 7	7 45	5·4	16 36	23 20	6 4	11 47 11	2 54 N.	9·8	17 42	23 59	6 17	13 44 40	10 50	4·2	20 49	1 56	7 3
28...	12 13 16	0 11 N.	4·8	17 42	23 48	5 54	12 32 45	2 9 S.	9·8	18 12	0 5	5 58	14 10 28	13 17	4·0	20 48	1 42	6 36
Oct. 8...	13 15 53	7 27 S.	4·8	18 41	0 8	5 35	13 18 38	7 10	9·8	18 44	0 11	5 38	14 37 13	15 35	4·0	20 47	1 29	6 11
18...	14 15 33	14 14	4·8	19 39	0 29	5 19	14 5 30	11 56	10·0	19 16	0 18	5 20	15 4 58	17 44	4·0	20 49	1 18	5 47
28...	15 14 22	19 45	5·0	20 30	0 48	5 6	14 53 57	16 14	10·0	19 48	0 27	5 6	15 33 47	19 38	4·0	20 49	1 7	5 25
Nov. 7...	16 12 17	23 38	5·6	21 14	1 6	4 58	15 44 22	19 50	10·2	20 21	0 38	4 55	16 3 40	21 17	4·0	20 51	0 58	5 5
17...	17 3 22	25 24	6·6	21 39	1 18	4 59	16 36 47	22 31	10·4	20 51	0 51	4 51	16 34 35	22 36	4·0	20 50	0 49	4 48
27...	17 24 44	24 29	8·6	21 16	1 0	4 44	17 30 49	24 7	10·6	21 18	1 6	4 54	17 6 25	23 34	4·0	20 50	0 42	4 34
Dec. 7...	16 43 43	20 28	9·8	19 19	23 30	3 41	18 25 40	24 30	10·8	21 36	1 22	5 8	17 39 1	24 8	4·0	20 47	0 35	4 23
17...	16 18 29	18 32	8·0	18 7	22 32	2 57	19 20 14	23 39	11·0	21 45	1 37	5 29	18 12 10	24 15	4·0	20 42	0 29	4 16
27...	16 49 40	20 42 S.	6·4	18 16	22 27	2 38	20 13 30	21 36 S.	11·4	21 46	1 51	5 56	18 45 36	23 56 S.	4·0	20 33	0 23	4 13

Mercury.

	d h	d h	d h	d h
Gr. elong. E....	Mar. 24 11;	July 21 23;	Nov. 16 4	
" W....	Jan. 11 14;	May 9 16;	Sept. 4 19;	Dec. 25 1
Sup. conj.	Feb. 27 3;	June 14 7;	Sept. 30 16;	
Inf. conj.	Apr. 11 20;	Aug. 18 21;	Dec. 5 17	
Stationary	Apr. 1 17;	Apr. 24 5;	Aug. 4 4;	Aug. 27 21

Venus.

	d h
Gr. elong. W...Feb.	10 19
Sup. conj. ......Sept.	15 20
Gr. brilliancy..Jan.	5 19

Superior Planets.

	Conjunction. d h	Opposition. d h	d h	Quadrature. d h	d h	Stationary. d h	d h
Mars ...	......	Jan. 18 12		Apr. 23 6	......	Feb. 27 3	
Jupiter...	Nov. 12 20	Apr. 25 7		Jan. 28 19;	July 24 1	Feb. 23 21;	June 27 16
Saturn ...	Dec. 17 17	June 11 2		Mar. 13 16;	Sept. 9 20	Apr. 2 0;	Aug. 21 9
Uranus...	Nov. 30 4	May 27 2		Feb. 26 6;	Aug. 27	Mar. 12 14;	Aug. 12 10

Jupiter (♃). Saturn (♄).

1899.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Se
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h
Jan. 1	14 16 34	12 26 S.	31·4	14 32	19 30	0 28	17 7 13	21 30 S.	14·0	18 15	22 20	2
21	14 26 7	13 11	33·2	13 27	18 21	23 15	17 16 17	21 41	14·2	17 7	21 11	1
Feb. 10	14 31 44	13 34	35·4	12 15	17 8	22 1	17 23 53	21 47	14·6	15 56	19 59	0
Mar. 2	14 32 41	13 34	37·6	10 57	15 50	20 43	17 29 24	21 50	15·0	14 45	18 46	22
22	14 28 49	13 12	39·6	9 33	14 27	19 21	17 32 21	21 51	15·6	13 27	17 30	21
Apr. 11	14 21 2	12 32	40·8	8 3	13 1	17 59	17 32 30	21 49	16·2	12 8	16 12	20
May 1	14 11 20	11 43	41·2	6 30	11 32	16 34	17 29 55	21 47	16·6	10 48	14 51	18
21	14 2 22	10 58	40·4	4 59	10 5	15 11	17 25 6	21 42	16·8	9 23	13 27	17
June 10	13 56 22	10 30	38·8	3 31	8 40	13 49	17 19 1	21 38	17·0	7 58	12 2	16
30	13 54 38	10 27	36·8	2 11	7 20	12 29	17 12 51	21 33	16·8	6 33	10 38	14
July 20	13 57 25	10 48	34·6	0 57	6 4	11 11	17 7 49	21 30	16·6	5 9	9 14	13
Aug. 9	14 4 20	11 30	32·8	23 49	4 53	9 57	17 4 53	21 30	16·2	3 47	7 52	11
29	14 14 46	12 29	31·2	22 45	3 44	8 43	17 4 33	21 33	15·6	2 29	6 34	10
Sept. 18	14 28 1	13 39	30·0	21 47	2 39	7 31	17 6 59	21 40	15·2	1 13	5 17	9
Oct. 8	14 43 23	14 55	29·0	20 51	1 36	6 21	17 11 57	21 50	14·8	0 1	4 4	8
28	15 0 15	16 11	28·6	19 56	0 34	5 12	17 19 6	22 1	14·4	22 50	2 52	6
Nov. 17	15 17 57	17 24	28·6	18 59	23 30	4 1	17 27 52	22 14	14·0	21 45	1 42	5
Dec. 7	15 35 48	18 29	28·8	18 5	22 29	2 53	17 37 40	22 19	14·0	20 32	0 33	4
27	15 53 1	19 25 S.	29·6	17 8	21 27	1 46	17 47 51	22 25 S.	14·0	19 22	23 21	3

Uranus (♅). Neptune (♆).

Jan. 1	16 16 23	21 12 S.	3·5	17 22	21 29	1 36	5 29 24	21 55 N.	2·7	6 41	10 44	14
31	16 22 4	21 25	3·5	15 30	19 37	23 42	5 26 32	21 54	2·7	4 40	8 43	12
Mar. 2	16 24 56	21 32	3·6	13 37	17 42	21 47	5 25 24	21 54	2·6	2 41	6 44	10
Apr. 1	16 24 27	21 31	3·6	11 38	15 43	19 48	5 26 31	21 57	2·6	0 44	4 47	8
May 1	16 21 2	21 24	3·7	9 36	13 42	17 48	5 29 25	22 0	2·6	22 50	2 52	6
31	16 15 59	21 12	3·7	7 32	11 39	15 46	5 33 44	22 4	2·5	20 57	0 59	5
June 30	16 11 12	21 0	3·6	5 28	9 36	13 44	5 38 31	22 7	2·5	18 59	23 1	3
July 30	16 8 23	20 56	3·6	3 27	7 36	11 45	5 42 53	22 9	2·5	17 7	21 8	1
Aug. 29	16 8 33	20 54	3·6	1 29	5 38	9 47	5 45 58	22 9	2·6	15 12	19 13	23
Sept. 28	16 12 54	21 3	3·5	23 35	3 43	7 51	5 47 10	22 8	2·6	13 15	17 16	21
Oct. 28	16 17 50	21 18	3·5	21 45	1 51	5 57	5 46 14	22 7	2·6	11 15	15 17	19
Nov. 27	16 25 19	21 36	3·4	19 54	{ 0 1 23 57 }	4 4	5 43 30	22 6	2·7	9 14	13 16	17
Dec. 27	16 33 0	21 53 S.	3·4	18 4	22 7	2 10	5 39 54	22 4 N.	2·7	7 13	11 15	15

Ceres. Pallas.

1899.	R.A. Noon.	Dec. Noon.	Transits.	1899.	R.A. Noon.	Dec. Noon.	Transits.
	h m	° ′	h m		h m	° ′	h m
Apr. 6 ...	14 39	1 59 S.	13 38	Mar. 13 ...	13 51	8 44 N.	14 25
18 ...	14 20	1 25	12 42	25 ...	13 45	12 58	13 32
30 ...	14 19	1 5	11 44	April 6 ...	13 37	16 50	12 36
May 12 ...	14 9	1 4 S.	10 47	18 ...	13 28	19 56	11 40
				30 ...	13 20	22 3 N.	10 45

Juno. Vesta.

May 24 ...	17 47	5 7 S.	13 37	Sept. 21 ...	1 56	0 20 S.	13 53
June 5 ...	17 38	4 36	12 41	Oct. 3 ...	1 47	1 33	12 56
17 ...	17 28	4 23	11 44	15 ...	1 36	2 40	11 58
29 ...	17 18	4 28 S.	10 46	27 ...	1 24	3 28	11 0
				Nov. 8 ...	1 15	3 48 S.	10 3

The times of rising and setting correspond to the adjacent transit, irrespective of the day given in the first column.

ECLIPSES IN 1899.

In the year 1899 there will be three Eclipses of the Sun and two of the Moon.

I.—*A Partial Eclipse of the Sun*, 1899, *January* 11.

Invisible at Greenwich. Greatest Eclipse Jan. 11^{d} 10^{h} 38^{m}·0. Magnitude (Sun's diam. = 1), 0·718. This Eclipse will be visible in Kamskatka, Alaska, and the North Pacific.

II.—*A Partial Eclipse of the Sun*, 1899, *June* 7.

Visible at Greenwich and throughout Northern Europe and Northern Asia. Greatest Eclipse June 7^{d} 18^{h} 34^{m}·0. Magnitude (Sun's diam. = 1), 0·611; in longitude 99° 5′ W., latitude 67° 18′ N.

Details for British Stations. The times are Greenwich Mean times, except for Dublin, where local mean time is used:—

Station.	Begins.	Greatest Eclipse.	Ends.	Magnitude.
	h m	h m	h m	
Greenwich	16 42·8	17 17·4	17 53·4	0·188
Cambridge	16 43·2	17 18·6	17 55·5	0·197
Oxford	16 42·8	17 18·2	17 55·1	0·200
Liverpool	16 43·8	17 21·5	18 0·9	0·233
Edinburgh	16 45·7	17 25·9	18 7·8	0·263
Dublin	16 18·6	16 57·3	17 37·6	0·253

At Greenwich (and approximately throughout the British Isles) first contact is 42° from N. point towards W., and last contact 29° from N. towards E. (direct image).

III.—*A Total Eclipse of the Moon*, 1899, *June* 22–23.

Invisible at Greenwich.

Greenwich M.T. of ☍ in R.A.: June 23^{d} 2^{h} 20^{m} 28^{s}·8.

	d h m		d h m
First contact with Penumbra	22 23 37·5	With shadow	23 0 32·8
Beginning of Total Phase	23 1 32·8	End	3 3·0
Second contact with Penumbra	4 58·3	With shadow	4 3·0

Magnitude of the Eclipse (Moon's diameter = 1), 1·487.

First contact with shadow 96° from N. point towards E.; last contact 109° towards W.

The Eclipse is visible in India, China, Australia, New Zealand, and the Pacific Ocean.

IV.—*An Annular Eclipse of the Sun*, 1899, *Dec.* 2.

Invisible at Greenwich. The central line passes over the Antarctic Circle.

A small partial eclipse is visible in S.W. Australia, Tasmania, and S. New Zealand.

V.—*A Partial (almost Total) Eclipse of the Moon*, 1899, *Dec.* 16.

Visible at Greenwich.

Greenwich M.T. of ☍ in R.A.: Dec. 16^{d} 13^{h} 29^{m} 4^{s}·7.

☾'s R.A. 5^{h} 37^{m} 55^{s}·01. Hourly motion 139^{s}·17. ☉'s 11^{s}·08.

	° ′ ″			′ ″	
☾'s Declination	22 55 37·3	N.	Hourly motion	1 43·2	S.
☉'s Declination	23 21 16·4	S.	Hourly motion	5·9	S.
☾'s Equat. Hor. Parallax	56 10·63		True Semidiameter..	15 18·61	
☉'s Equat. Hor. Parallax	8·99		True Semidiameter..	16 15·38	

	h m		h m
First contact with Penumbra	10 33·7	With shadow	11 44·6
Second „ „ „	16 18·1	„ „	15 7·2

Magnitude of the Eclipse (Moon's diam. = 1), 0·995.

First contact with the shadow at 66° from N. point towards E.; last contact at 59° towards W.

The Eclipse is visible in Western Asia, throughout Europe and Africa, and in Eastern America.

Date.	Star.	Mag.	Disap.		Reap.	
			M.T.	P.	M.T.	P.
Jan.			h m	°	h m	°
3	D.M.−6° 3518	6·4	18 50	111	20 4	308
5	D.M.−15° 3715	6·5	13 33†	139	14 29	273
7	42 Libræ	5·2	17 3	60	17 48	335
19	*μ* Arietis.........	5·8	8 5	45	9 14	282
22	121 Tauri	5·4	15 33	99	16 29	279
24	56 Geminorum .	5·0	17 32	108	18 26	286
29	*p*² Leonis	6·2	12 22	185	12 57	240
30	B.A.C. 4006 ...	5·7	13 39	126	14 57	302
Feb.						
1	D.M.−13° 3692	6·5	11 35*	68	12 16	346
12	19 Piscium......	5·2	7 35	78	8 28‡	235
18	103 Tauri	5·5	8 53	96	10 11	273
19	3 Geminorum ..	6·5	13 57	152	14 34	232
22	B.A.C. 2888 ...	6·5	12 42	134	13 51	280
25	36 Sextantis ...	6·5	7 22	133	8 25	280
25	55 Leonis	6·0	15 20	164	16 11	256
Mar.						
3	B.A.C. 5254 ...	5·4	13 5†	74	14 2	318
20	56 Geminorum .	5·0	5 32	124	6 50	265
20	61 Geminorum .	5·7	8 50	83	9 59	320
25	*e* Leonis	5·1	16 52	90	17 45†	316
26	D.M.−6° 3518 .	6·4	13 57	202	14 8	218
27	D.M.−10° 3570	6·0	10 0	81	10 56	340
28	83 Virginis......	5·8	9 57	77	10 49	339
29	B.A.C. 4867 ...	6·4	15 13	148	16 6	244
29	B.A.C. 4879 ...	6·3	16 12	184	16 28	208
30	D.M.−22° 3989	6·0	11 59	53	12 40	343
Apr.						
4	B.A.C. 7145 ...	6·0	14 52†	349	14 58†	340
15	*μ* Geminorum ..	3·2	11 26	65	12 8	319
17	3 Cancri	6·0	10 44	182	11 8	224
19	*h* Leonis	5·4	9 10	154	10 14	267
21	*p*² Leonis	6·2	10 1	105	11 14	319
22	B.A.C. 4006 ...	5·7	11 12	95	12 20	325
24	D.M.−13° 3692	6·5	7 9	127	8 14	288
25	D.M.−17° 4053	6·5	8 19	64	9 1	346
25	B.A.C. 4739 ...	6·2	9 17	49	9 45	1
26	B.A.C. 5023 ...	5·8	9 53	122	11 1	277
28	*θ* Ophiuchi ...	3·4	11 56	89	13 6	280
30	B.A.C. 6671 ...	6·5	15 25	138	15 57	189
May	[(2nd *)					
23	B.A.C. 4923	6·3	11 58	59	12 51	330
24	B.A.C. 5254 ...	5·4	10 15	125	11 25	261
25	B.A.C. 5709 ...	6·3	11 59	106	13 12	259
25	26 Ophiuchi ...	6·1	12 4	83	13 18	281
26	7 Sagittarii ...	5·4	11 39	51	12 39	302
26	9 Sagittarii ...	5·7	12 3	72	13 15	278
26	B.A.C. 6111 ...	6·5	12 41	85	13 56	263
une						
1	19 Piscium......	5·2	14 53	20	15 40	284
18	D.M.−15° 3715	6·5	12 7	125	13 6†	267
19	B.A.C. 4814 ...	6·4	12 57‡	139	13 47†	242
23	B.A.C. 6343 ...	5·8	10 34	102	11 41	240
24	*f* Sagittarii ...	5·1	13 17	30	14 12	290
25	B.A.C. 7145 ...	6·0	9 50	92	10 50	232
26	D.M.−11° 5640	6·3	10 45	65	11 48	250
27	*κ* Aquarii	5·5	12 59	40	14 2	262
28	*κ* Piscium	5·0	11 22	32	12 10	278

Date.	Star.	Mag.	Disap.	
			M.T.	P.
July			h m	°
3	D.M.+21° 539 .	5·7	12 40†	83
3	32 Tauri	5·7	14 57	39
19	B.A.C. 5709 ...	6·3	8 6	113
19	26 Ophiuchi ...	6·1	8 9	90
20	7 Sagittarii ...	5·4	7 39	58
20	9 Sagittarii ...	5·7	8 4	77
20	B.A.C. 6111 ...	6·5	8 41	89
23	D.M.−13° 5891	6·5	10 59	148
24	B.A.C. 7717 ...	6·5	8 53	63
31	D.M.+23° 715 .	6·5	14 6	24
Aug.				
14	D.M.−22° 3989	6·0	8 25	94
17	B.A.C. 6343 ...	5·8	7 36	134
18	*f* Sagittarii	5·1	10 28	50
18	57 Sagittarii ...	6·2	13 43	13
19	B.A.C. 7145 ...	6·0	6 49	87
20	D.M. − 11° 5640	6·3	7 2	46
22	16 Piscium......	5·6	9 21	33
22	19 Piscium......	5·2	15 1	48
26	*τ*² Arietis	5·2	12 5	91
26	65 Arietis	5·6	12 55	106
27	B.A.C. 1289 ...	6·2	9 55	69
27	*υ*¹ Tauri	4·6	16 20	122
27	*υ*² Tauri	5·5	16 49	87
29	*η* Geminorum...	Var.	16 9	92
30	*ζ* Geminorum...	Var.	14 59	120
Sept.				
12	39 Ophiuchi ...	6·0	8 47	28
13	1 Sagittarii......	5·3	5 5	51
17	B.A.C. 7717 ...	6·5	6 18	64
23	A¹ Tauri.........	4·5	15 29	125
Oct.				
10	4 Sagittarii......	4·6	7 3	96
12	57 Sagittarii ...	6·2	4 19	58
16	16 Piscium......	5·6	6 3	41
16	19 Piscium......	5·2	11 53	52
19	27 Arietis	6·5	9 22	78
21	*κ*¹ Tauri	4·6	8 37	76
21	*κ*² Tauri	5·5	8 38	97
Nov.				
12	*κ* Piscium	5·0	11 11	136
17	A¹ Tauri.........	4·5	10 21	54
17	A² Tauri.........	6·5	10 36	76
17	B.A.C. 1289 ...	6·2	15 10	23
19	Neptune	...	6 10	95
19	B.A.C. 1970 ...	6·5	16 20	29
21	B.A.C. 2649 ...	6·3	19 10	183
25	55 Leonis	6·0	14 11	108
Dec.				
14	D.M.+20° 602 .	6·2	7 43	148
15	*κ*² Tauri	5·5	2 48	57
15	*κ*¹ Tauri	4·6	2 55	31
16	Neptune	...	15 36	158
18	*f* Geminorum...	5·2	18 31	161
19	D.M.+16° 1679	6·5	11 0	92
25	D.M −10° 3570	6·0	14 39	128
26	83 Virginis	5·8	16 22	172

* Star rising. ‡ Star setting. † Star below horizon.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i. e.* from *bottom of* the Moon's inverted image towards the right.

The following "near approaches" are also given in the 'Nautical Almanac,' and are especially interesting to observers rather South and rather North of Greenwich respectively:—

North Near Approaches.

Date.	Star.	Mag.	G.M.T.	Angle.
Jan.			h m	°
6	B.A.C. 4867 ...	6·4	20 20	18
Feb.				
7	B.A.C. 6671 ...	6·5	18 30	350
Mar.				
2	B.A.C. 4923 (2nd *).	6·3	13 39	22
2	D.M. − 21°4030	6·5	19 7	11
6	B.A.C. 6485 ...	6·5	15 49	354
17	99 Tauri	6·0	12 57	4
Apr.				
5	D.M. − 11°5640	6·3	16 45	338
23	*q* Virginis	5·7	8 9	31
24	75 Virginis ...	5·7	14 15	20
May				
14	*g* Geminorum ..	5·1	11 29	17
19	*e* Leonis.........	5·1	9 28	31
June				
18	75 Virginis ...	5·7	7 58	26
July			h m	°
3	B.A.C. 1189 ...	6·0	12 31	349
10	*h* Leonis.........	5·4	8 35	23
Aug.				
2	*μ* Geminorum .	3·2	14 14	0
21	*κ* Aquarii	5·5	8 19	335
Oct.				
14	c^2 Capricorni...	6·2	6 1	334
20	65 Arietis	5·6	7 56	344
21	*τ* Tauri	4·4	18 2	2
Nov.				
9	τ^2 Capricorni...	5·3	7 49	334
16	τ^2 Arietis	5·2	18 52	355
16	65 Arietis	5·6	19 31	355
Dec.				
23	*e* Leonis.........	5·1	19 36	29

South Near Approaches.

Date.	Star.	Mag.	G.M.T.	Angle.
Jan.			h m	°
26	B.A.C. 2888 ...	6·5	4 54	193
Feb.				
19	1 Geminorum ..	4·3	11 26	192
Mar.				
3	B.A.C. 5253 ...	5·6	13 49	197
16	32 Tauri	5·7	10 15	178
30	42 Libræ	5·2	14 1	196
Apr.				
2	26 Sagittarii ...	6·2	13 56	177
13	v^2 Tauri.........	5·5	8 35	182
15	*η* Geminorum ..	Var.	8 6	194
May				
27	B.A.C. 6485 ...	6·5	10 10	174
Aug.			h m	°
29	B.A.C. 1970 ...	6·5	13 59	179
Sept.				
23	A^2 Tauri	6·5	16 17	172
Oct.				
20	D.M. + 20°602..	6·2	15 16	171
21	56 Tauri.........	5·4	6 42	170
25	5 Cancri.........	6·4	10 6	189
Nov.				
17	56 Tauri.........	5·4	18 30	181
21	*f* Geminorum ..	5·2	8 3	188
Dec.				
19	29 Cancri	5·9	18 53	204
24	D.M. − 6·3518 ..	6·4	16 56	211

TABLE OF BESSEL'S MEAN REFRACTIONS.

Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.
°	′ ″	°	′ ″	°	′ ″	°	′ ″
0	0·0	56	1 25·4	68	2 21·9	78	4 25·0
10	10·2	58	1 32·1	69	2 29·3	79	4 48·5
20	21·0	60	1 39·7	70	2 37·3	80	5 16·2
30	33·3	61	1 43·8	71	2 46·1	81	5 49·3
35	40·4	62	1 48·2	72	2 55·8	82	6 29·6
40	48·4	63	1 52·8	73	3 6·6	83	7 19·7
45	57·7	64	1 57·8	74	3 18·6	84	8 23·3
50	1 8·7	65	2 3·2	75	3 32·1	85	9 46·5
52	1 13·8	66	2 8·9	76	3 47·4		
54	1 19·3	67	2 15·2	77	4 4·9		

Approximate Times and Position-angles of Stars which will be occulted (as seen from Greenwich) during the almost Total Lunar Eclipse of 1899, Dec. 16.

Star.	Mag.	Disapp. or Reapp.	Mean Time. h m s	Sidereal Time. h m s	P. °	V. °
B.D. 22° 993...	8·1	D.	12 17 13	5 58 53	92	83
22° 991...	8·6	D.	12 18 45	6 0 26	61	52
22° 996...	6·5	D.	12 23 7	6 4 48	65	54
22° 999...	9·0	D.	12 25 18	6 6 59	81	70
22° 998...	9·5	D.	12 27	6 9	61	50
22°1000...	8·5	D.	12 37 2	6 18 46	132	119
22°1003...	8·4	D.	12 55 1	6 37 35	106	88
22°1004...	8·3	D.	13 1 33	6 43 20	54	34
22°1006...	8·8	D.	13 9 16	6 51 4	46	24
22° 991...	8·6	R.	13 20 42	7 2 32	312	288
22°1011...	9·5	D.	13 21	7 3	79	55
22°1009...	9·4	D.	13 29	7 11	132	106
22°1012...	9·5	D.	13 30	7 12	48	22
22° 996...	6·5	R.	13 31 10	7 13 2	307	281
22° 998...	9·5	R.	13 32	7 14	312	286
22° 993...	8·1	R.	13 35 41	7 17 33	281	254
22°1000...	8·5	R.	13 40 16	7 22 9	240	212
22° 999...	9·0	R.	13 40 44	7 22 38	293	265
22°1014...	9·5	D.	13 49	7 31	82	53
22°1006...	8·8	R.	13 57 53	7 39 49	331	300
22°1004...	8·3	R.	13 58 25	7 40 22	315	284
22°1016...	9·5	D.	13 59	7 41	84	53
22°1012...	9·5	R.	14 19	8 1	329	295
22°1009...	9·4	R.	14 31	8 13	246	210
22°1011...	9·5	R.	14 31	8 13	298	262

Neptune is occulted at Greenwich shortly after the eclipse is over.

CONFIGURATIONS OF JUPITER'S SATELLITES

FOR AN INVERTING TELESCOPE.

Day.	Jan. 17h	Feb. 16h	March. 14h	April. 13h	May. 12h	June. 11h	July. 9½h	Aug. 8½h	Sept. 7h	Oct. 6h	Dec. 19h	Day.
1	O1234	1O234	412O3	432O1	3O124	21O34	1O234	342O1	4213O	4O123		1
2	2O34●	♃3O24	4O312	4312O	143O2	O1234	♃2O14	31O42	42O13	4O23●		2
3	21O34	32O14	♃431O	43O12	42O13	1O324	321O4	32O14	41O23	421O3		3
4	♃O142	321O4	324O1	♃41O3	412O3	234O1	3O124	21O34	42O13	432O1		4
5	♃314O	3O124	3O24●	42O13	4O123	3412O	31O24	O1234	213O4	431O2		5
6	342O1	1O234	1O24●	4O123	432O●	♃43O2	2O134	1O234	3O124	♃43O1		6
7	431O2	2O413	2O134	1O342	4321O	42O31	21O43	23O14	31O24	241O●		7
8	4O132	41O23	12O34	32O14	43O12	421O3	41O23	32O4●	♃23O4	O4123		8
9	421O3	43O12	O1324	312O4	413O2	4O213	42O31	31O42	2O134	O243●		9
10	♃42O3	432O●	31O24	3O124	2O413	41O32	4321O	♃34O1	1O234	21O34		10
11	4O312	4321O	32O14	1O234	12O43	423O1	43O21	421O3	♃O134	23O14	41O23	11
12	431O2	43O12	31O24	2O134	O1234	3214O	431O2	4O213	213O4	31O24	4O213	12
13	324O1	41O23	♃4O2●	O234●	♃13O4	3O142	42O1●	41O23	34O21	3O214	24O3●	13
14	31O24	42O13	42O13	1O324	♃32O4	♃3O4●	421O3	42O31	341O2	213O4	31O24	14
15	O3124	41O23	421O3	32O41	3O124	21O34	♃4O23	432O●	423O1	O2134	3O124	15
16	21O34	♃O142	4O123	3421O	31O24	O2134	♃O143	431O2	42O3●		321O4	16
17	2O134	32O4●	413O2	43O12	2O134	1O234	231O4	43O21	41O23		23O14	17
18	O1324	321O4	432O1	41O32	12O43	23O14	3O214	214O3	4O213		1O234	18
19	31O24	3O124	431O●	42O13	4O123	321O4	31O24	O143●	♃421O		O2134	19
20	32O14	1O234	43O12	41O23	41O32	3O142	2O314	1O234	34O1●		21O34	20
21	31O4●	2O134	24O3●	♃4O32	432O1	431O2	21O34	2O314	31O42		♃3O4●	21
22	4O312	1O34●	21O43	432O1	43O●●	421O3	O1234	321O4	32O14		3O412	22
23	412O3	O3124	O1234	3214O	431O2	4O13●	O234●	♃3O24	21O34		3412O	23
24	42O13	3214O	13O24	3O142	42O31	41O23	231O4	3O124	1O234		432O1	24
25	4O32●	♃342O	32O14	1O24●	421O3	423O1	34O21	21O4●	O1234		41O32	25
26	431O2	43O12	31O4●	2O134	4O123	4321O	431O2	O143●	21O34		4O213	26
27	432O1	41O32	3O124	1O34●	1O324	43O12	423O1	14O23	32O14		421O3	27
28	431O●	42O13	2O34●	O1324	32O14	431O2	421O3	42O13	31O24		4O13●	28
29	43O12		21O43	32O14	3O4●●	♃2O43	4O123	4231O	32O41		43O2●	29
30	12O43		4O123	321O4	31O24	O43●●	41O23	43O12	241O3		3412O	30
31	2O143		♃41O2		2O314		4231O	43O2●			32O41	31

The circle (O) represents Jupiter; ♃ signifies that the satellite is on the disk; ● signifies that the satellite is behind the disk or in the shadow.

SATELLITES OF MARS

DURING THE OPPOSITION OF 1898–9.

East Elongations.

Phobos.

January.		Jan. (*con.*).		Jan. (*con.*).		Jan. (*con.*).		Feb. (*con.*).		Feb. (*con.*).	
d	h	d	h	d	h	d	h	d	h	d	h
1	7·2	9	14·2	17	21·1	26	4·0	3	3·3	11	10·3
	14·9		21·8	18	4·8		11·7		11·0		18·0
	22·5	10	5·5		12·4		19·3		18·6	12	1·6
2	6·2		13·1		20·1	27	3·0	4	2·3		9·3
	13·9		20·8	19	3·7		10·6		9·9		16·9
	21·5	11	4·4		11·4		18·3		17·6	13	0·6
3	5·2		12·1		19·0	28	1·9	5	1·3		8·2
	12·8		19·7	20	2·7		9·6		8·9		15·9
	20·5	12	3·4		10·3		17·2		16·6		23·5
4	4·1		11·0		18·0	29	0·9	6	0·2	14	7·2
	11·8		18·7	21	1·6		8·6		7·9		14·8
	19·4	13	2·3		9·3		16·2		15·5		22·5
5	3·1		10·0		16·9		23·9		23·2	15	6·1
	10·7		17·7	22	0·6	30	7·5	7	6·8		13·8
	18·4	14	1·3		8·2		15·2		14·5		21·4
6	2·0		9·0		15·9		22·8		22·1	16	5·1
	9·7		16·6		23·5	31	6·5	8	5·8		12·7
	17·3	15	0·3	23	7·2		14·1		13·4		20·4
7	1·0		7·9		14·8		21·8		21·1	17	4·0
	8·6		15·6		22·5	February.		9	4·7		11·7
	16·3		23·2	24	6·1	1	5·4		12·4		19·3
	23·9	16	6·9		13·8		13·1		20·0	18	3·0
8	7·6		14·5		21·5		20·7	10	3·7		10·7
	15·2		22·2	25	5·1	2	4·4		11·3		18·3
	22·9	17	5·8		12·7		12·0		19·0		
9	6·5		13·5		20·4		19·7	11	2·6		

Deimos.

January.		Jan. (*con.*).		Jan. (*con.*).		Jan. (*con.*).		Feb. (*con.*).		Feb. (*con.*).	
d	h	d	h	d	h	d	h	d	h	d	h
1	4·3	11	6·5	21	8·5	30	4·4	5	11·7	15	13·9
2	10 6	12	12·7	22	14·8	31	10·6	6	18·0	16	20·2
3	16·9	13	19·0	23	21·1			8	0·3	18	2·5
4	23·1	15	1·3	25	3·4	February.		9	6·6	19	8·8
6	5·4	16	7·5	26	9·6			10	12·9	20	15·1
7	11·7	17	13·8	27	15·8	1	16·9	11	19·1	21	21·4
8	17·9	18	20·0	28	22·1	2	23·2	13	1·4	23	3·7
10	0·2	20	2·2			4	5·4	14	7·7	24	10·0

Apparent Discs of Venus and Mars, 1899.

	Venus.	*Mars.*		*Venus.*	*Mars.*		*Venus.*	*Mars.*
Jan. 15...	0·332	0·999	May 15...	0·834	0·901	Sept. 15...	1·000	0·967
Feb. 14...	0·517	0·969	June 15...	0·904	0·913	Oct. 15...	0·991	0·981
Mar. 15...	0·644	0·922	July 15...	0·955	0·931	Nov. 15...	0·966	0·991
April 15...	0·750	0·900	Aug. 15...	0·989	0·949	Dec. 15...	0·928	0·998

The numbers in this table are the versed sines of the illuminated disk, the apparent diameter of the planet being taken as unity.

JUPITER'S SATELLITES, 1899.

Phenomena.

e, Signifies Ec. dis.; E, Ec. re.; *o*, Occ. dis.; O, Occ. re. *t*, Tr. Ing.; T, Tr. Egr.

January.

d	h	m	
1	1	43	iii. *o*.
	3	32	iii. O.
	18	56	i. *t*.
	21	8	i. T.
2	3	32	ii. *t*.
	5	52	ii. T.
	15	11	i. *e*.
	18	29	i. O.
3	13	25	i. *t*.
	15	37	i. T.
	19	33	ii. *e*.
4	0	6	ii. O.
	9	39	i. *e*.
	12	58	i. O.
	15	38	iii. *t*.
	17	24	iii. T.
5	7	54	i. *t*.
	10	6	i. T.
	16	53	ii. *t*.
	19	12	ii. T.
6	4	7	i. *e*.
	7	27	i. O.
7	2	23	i. *t*.
	4	35	i. T.
	8	49	ii. *e*.
	11	6	ii. E.
	11	7	ii. *o*.
	13	26	ii. O.
	22	36	i. *e*.
8	1	11	iii. *e*.
	1	56	i. O.
	3	0	iii. E.
	5	56	iii. *o*.
	7	39	iii. O.
	20	52	i. *t*.
	23	4	i. T.
9	6	14	ii. *t*.
	8	32	ii. T.
	17	4	i. *e*.
	20	25	i. O.
10	15	21	i. *t*.
	17	33	i. T.
	22	6	ii. *e*.
11	0	23	ii. E.
	0	26	ii. *o*.
	2	45	ii. O.
	11	32	i. *e*.
	14	54	i. O.
	19	48	iii. *t*.
	21	29	iii. T.
12	9	50	i. *t*.
	12	2	i. T.
	19	33	ii. *t*.
	21	52	ii. T.

Jan. (con.).

d	h	m	
13	6	1	i. *e*.
	9	22	i. O.
14	4	19	i. *t*.
	6	31	i. T.
	11	23	ii. *e*.
	13	40	ii. E.
	13	46	ii. *o*.
	16	4	ii. O.
15	0	29	i. *e*.
	3	51	i. O.
	5	9	iii. *e*.
	6	56	iii. E.
	10	5	iii. *o*.
	11	43	iii. O.
	22	47	i. *t*.
16	0	59	i. T.
	8	53	ii. *t*.
	11	11	ii. T.
	18	57	i. *e*.
	22	20	i. O.
17	17	16	i. *t*.
	19	28	i. T.
18	0	40	ii. *e*.
	2	56	ii. E.
	3	5	ii. *o*.
	5	22	ii. O.
	13	26	i. *e*.
	16	48	i. O.
	23	55	iii. *t*.
19	1	31	iii. T.
	11	45	i. *t*.
	13	57	i. T.
	22	12	ii. *t*.
20	0	29	ii. T.
	7	54	i. *e*.
	11	17	i. O.
21	6	13	i. *t*.
	8	25	i. T.
	13	57	ii. *e*.
	16	13	ii. E.
	16	23	ii. *o*.
	18	40	ii. O.
22	2	22	i. *e*.
	5	46	i. O.
	9	7	iii. *e*.
	10	53	iii. E.
	14	10	iii. *o*.
	15	43	iii. O.
23	c	42	i. *t*.
	2	54	i. T.
	11	31	ii. *t*.
	13	48	ii. T.
	20	51	i. *e*.
24	0	14	i. O.

Jan. (con.).

d	h	m	
24	19	10	i. *t*.
	21	22	i. T.
25	3	14	ii. *e*.
	5	30	ii. E.
	5	41	ii. *o*.
	7	58	ii. O.
	15	19	i. *e*.
	18	42	i. O.
26	3	59	iii. *t*.
	5	30	iii. T.
	13	39	i. *t*.
	15	50	i. T.
27	0	48	ii. *t*.
	3	5	ii. T.
	9	47	i. *e*.
	13	11	i. O.
28	8	7	i. *t*.
	10	19	i. T.
	16	31	ii. *e*.
	18	47	ii. E.
	18	58	ii. *o*.
	21	15	ii. O.
29	4	15	i. *e*.
	7	39	i. O.
	13	4	iii. *e*.
	14	49	iii. E.
	18	12	iii. *o*.
	19	40	iii. O.
30	2	36	i. *t*.
	4	47	i. T.
	14	6	ii. *t*.
	16	22	ii. T.
	22	44	i. *e*.
31	2	7	i. O.
	21	4	i. *t*.
	23	15	i. T.

February.

d	h	m	
1	5	47	ii. *e*.
	8	4	ii. E.
	8	16	ii. *o*.
	10	31	ii. O.
	17	12	i. *e*.
	20	36	i. O.
2	7	59	iii. *t*.
	9	24	iii. T.
	15	32	i. *t*.
	17	43	i. T.
3	3	23	ii. *t*.
	5	39	ii. T.
	11	40	i. *e*.
	15	4	i. O.
4	10	0	i. *t*.

Feb. (con.).

d	h	m	
4	12	11	i. T.
	19	5	ii. *e*.
	21	21	ii. E.
	21	32	ii. *o*.
	23	48	ii. O.
5	6	9	i. *e*.
	9	32	i. O.
	17	2	iii. *e*.
	18	46	iii. E.
	22	9	iii. *o*.
	23	32	iii. O.
6	4	28	i. *t*.
	6	39	i. T.
	16	40	ii. *t*.
	18	55	ii. T.
7	0	37	i. *e*.
	4	0	i. O.
	22	56	i. *t*.
8	1	7	i. T.
	8	21	ii. *e*.
	10	38	ii. E.
	10	48	ii. *o*.
	13	3	ii. O.
	19	5	i. *e*.
	22	28	i. O.
9	11	54	iii. *t*.
	13	15	iii. T.
	17	24	i. *t*.
	19	35	i. T.
10	5	55	ii. *t*.
	8	10	ii. T.
	13	34	i. *e*.
	16	55	i. O.
11	11	52	i. *t*.
	14	3	i. T.
	21	39	ii. *e*.
	23	55	ii. E.
12	0	4	ii. *o*.
	2	19	ii. O.
	8	2	i. *e*.
	11	23	i. O.
	21	0	iii. *e*.
	22	43	iii. E.
13	2	2	iii. *o*.
	3	20	iii. O.
	6	20	i. *t*.
	8	31	i. T.
	19	10	ii. *t*.
	21	25	ii. T.
14	2	31	i. *e*.
	5	51	i. O.
15	0	47	i. *t*.
	2	58	i. T.
	10	56	ii. *e*.

Feb. (con.).

d	h	m	
15	13	12	ii. E.
	13	19	ii. *o*.
	15	34	ii. O.
	20	58	i. *e*.
16	0	19	i. O.
	15	45	iii. *t*.
	17	0	iii. T.
	19	15	i. *t*.
	21	26	i. T.
17	8	25	ii. *t*.
	10	39	ii. T.
	15	27	i. *e*.
	18	46	i. O.
18	13	43	i. *t*.
	15	54	i. T.
19	0	13	ii. *e*.
	2	29	ii. E.
	2	34	ii. *o*.
	4	48	ii. O.
	9	55	i. *e*.
	13	14	i. O.
20	0	57	iii. *e*.
	2	39	iii. E.
	5	51	iii. *o*.
	7	4	iii. O.
	8	10	i. *t*.
	10	21	i. T.
	21	39	ii. *t*.
	23	53	ii. T.
21	4	23	i. *e*.
	7	41	i. O.
22	2	38	i. *t*.
	4	49	i. T.
	13	30	ii. *e*.
	15	46	ii. E.
	15	48	ii. *o*.
	18	2	ii. O.
	22	52	i. *e*.
23	2	8	i. O.
	19	30	iii. *t*.
	20	41	iii. T.
	21	5	i. *t*.
	23	16	i. T.
24	10	53	ii. *t*.
	13	6	ii. T.
	17	20	i. *e*.
	20	36	i. O.
25	15	32	i. *t*.
	17	43	i. T.
26	2	48	ii. *e*.
	7	15	ii. O.
	11	48	i. *e*.
	15	3	i. O.
27	4	56	iii. *e*.

Feb. (con.).

d	h	m	
27	6	37	iii.
	9	35	iii.
	9	59	i. *t*.
	10	43	iii.
	12	10	i. T
28	0	6	ii. *t*
	2	19	ii. T
	6	16	i. *e*.
	9	30	i. O

March.

d	h	m	
1	4	27	i. *t*.
	6	38	i. T
	16	5	ii. *e*
	20	28	ii. O
2	0	45	i. *e*.
	3	57	i. O
	22	54	i. *t*.
	23	12	iii.
3	0	18	iii.
	1	5	i. T
	13	18	ii. *t*
	15	31	ii. T
	19	13	i. *e*.
	22	24	i. O
4	17	21	i. *t*.
	19	32	i. T
5	5	22	ii. *e*
	9	40	ii. O
	13	41	i. *e*.
	16	51	i. O
6	8	53	iii.
	10	33	iii.
	11	48	i. *t*.
	13	14	iii.
	13	59	i. T
	14	18	iii.
7	2	29	ii. *t*
	4	42	ii. T
	8	10	i. *e*.
	11	18	i. O
8	6	15	i. *t*.
	8	26	i. T
	18	40	ii. *e*
	22	51	ii. O
9	2	38	i. *e*.
	5	45	i. O
10	0	42	i. *t*.
	2	48	iii.
	2	53	i. T
	3	51	ii
	15	40	ii.
	17	53	ii. T

Mar. (con.).

d	h	m	
10	21	6	i. *e.*
[illegible]	0	12	i. O.
	19	9	i. *t.*
	21	19	i. T.
12	7	57	ii. *e.*
	12	2	ii. O.
	15	35	i. *e.*
	18	38	i. O.
13	12	51	iii. *e.*
	13	35	i. *t.*
	14	30	iii. E.
	15	46	i. T.
	16	48	iii. *o.*
	17	49	iii. O.
14	4	51	ii. *t.*
	7	3	ii. T.
	10	3	i. *e.*
	13	5	i. O.
15	8	2	i. *t.*
	10	13	i. T.
	21	15	ii. *e.*
16	1	13	ii. O.
	4	31	i. *e.*
	7	32	i. O.
17	2	29	i. *t.*
	4	40	i. T.
	6	20	iii. *t.*
	7	20	iii. T.
	18	0	ii. *t.*
	20	13	ii. T.
	22	59	i. *e.*
18	1	58	i. O.
	20	55	i. *t.*
	23	6	i. T.
19	10	33	ii. *e.*
	14	24	ii. O.
	17	28	i. *e.*
	20	25	i. O.
20	15	22	i. *t.*
	16	49	iii. *e.*
	17	33	i. T.
	18	27	iii. E.
	20	17	iii. *o.*
	21	16	iii. O.
21	7	10	ii. *t.*
	9	22	ii. T.
	11	56	i. *e.*
	14	51	i. O.
22	9	48	i. *t.*
	11	59	i. T.
	23	50	ii. *e.*
23	3	33	ii. O.
	6	24	i. *e.*
	9	17	i. O.
24	4	15	i. *t.*
	6	26	i. T.
	9	48	iii. *t.*
	10	47	iii. T.
	20	19	ii. *t.*
	22	31	ii. T.
2[illegible]	0	53	i. *e.*
	3	44	i. O.
	22	41	i. *t.*
26	0	52	i. T.

Mar. (con.).

d	h	m	
26	13	8	ii. *e.*
	16	43	ii. O.
	19	21	i. *e.*
	22	10	i. O.
27	17	8	i. *t.*
	19	18	i. T.
	20	46	iii. *e.*
	22	24	iii. E.
	23	41	iii. *o.*
28	0	40	iii. O.
	9	27	ii. *t.*
	11	39	ii. T.
	13	49	i. *e.*
	16	36	i. O.
29	11	34	i. *t.*
	13	44	i. T.
30	2	25	ii. *e.*
	5	52	ii. O.
	8	18	i. *e.*
	11	2	i. O.
31	6	0	i. *t.*
	8	11	i. T.
	13	11	iii. *t.*
	14	10	iii. T.
	22	35	ii. *t.*

April.

d	h	m	
1	0	47	ii. T.
	2	46	i. *e.*
	5	28	i. O.
2	0	27	i. *t.*
	2	37	i. T.
	15	44	ii. *e.*
	19	1	ii. O.
	21	14	i. *e.*
	23	55	i. O.
3	18	53	i. *t.*
	21	3	i. T.
4	0	45	iii. *e.*
	2	21	iii. E.
	3	2	iii. *o.*
	4	1	iii. O.
	11	42	ii. *t.*
	13	54	ii. T.
	15	43	i. *e.*
	18	21	i. O.
5	13	19	i. *t.*
	15	29	i. T.
6	5	1	ii. *e.*
	8	9	ii. O.
	10	11	i. *e.*
	12	47	i. O.
7	7	45	i. *t.*
	9	55	i. T.
	16	30	iii. *t.*
	17	31	iii. T.
8	0	50	ii. *t.*
	3	2	ii. T.
	4	40	i. *e.*
	7	13	i. O.
9	2	11	i. *t.*
	4	21	i. T.
	18	20	ii. *e.*

Apr. (con.).

d	h	m	
9	21	18	ii. O.
	23	8	i. *e.*
10	1	39	i. O.
	20	37	i. *t.*
	22	48	i. T.
11	4	43	iii. *e.*
	6	18	iii. E.
	6	20	iii. *o.*
	7	22	iii. O.
	13	57	ii. *t.*
	16	9	ii. T.
	17	36	i. *e.*
	20	5	i. O.
12	15	3	i. *t.*
	17	14	i. T.
13	7	37	ii. *e.*
	10	26	ii. O.
	12	5	i. *e.*
	14	30	i. O.
14	9	29	i. *t.*
	11	40	i. T.
	19	47	iii. *t.*
	20	50	iii. T.
15	3	3	ii. *t.*
	5	16	ii. T.
	6	33	i. *e.*
	8	56	i. O.
16	3	55	i. *t.*
	6	6	i. T.
	20	56	ii. *e.*
	23	34	ii. O.
17	1	2	i. *e.*
	3	22	i. O.
	22	21	i. *t.*
18	0	32	i. T.
	8	42	iii. *e.*
	10	41	iii. O.
	16	10	ii. *t.*
	18	22	ii. T.
	19	30	i. *e.*
	21	48	i. O.
19	16	47	i. *t.*
	18	58	i. T.
20	10	13	ii. *e.*
	12	42	ii. O.
	13	58	i. *e.*
	16	14	i. O.
21	11	13	i. *t.*
	13	24	i. T.
	23	1	iii. *t.*
22	0	10	iii. T.
	5	16	ii. *t.*
	7	29	ii. T.
	8	27	i. *e.*
	10	40	i. O.
23	5	39	i. *t.*
	7	50	i. T.
	23	32	ii. *e.*
24	1	50	ii. O.
	2	55	i. *o.*
	5	6	i. O.
25	0	5	i. *t.*
	2	16	i. T.
	12	40	iii. *e.*

Apr. (con.).

d	h	m	
25	14	14	iii. E.
	18	23	ii. *t.*
	20	36	ii. T.
	21	21	i. *o.*
	23	32	i. O.
26	18	31	i. *t.*
	20	42	i. T.
27	12	44	ii. *o.*
	15	5	ii. E.
	15	47	i. *o.*
	17	59	i. E.
28	12	57	i. *t.*
	15	8	i. T.
29	2	16	iii *t.*
	3	30	iii. T.
	7	29	ii. *t.*
	9	42	ii. T.
	10	13	i. *o.*
	12	28	i. E.
30	7	23	i. *t.*
	9	34	i. T.

May.

d	h	m	
1	1	52	ii. *o.*
	4	24	ii. E.
	4	39	i. *o.*
	6	56	i. E.
2	1	49	i. *t.*
	4	0	i. T.
	16	3	iii. *o.*
	18	11	iii. E.
	20	35	ii. *t.*
	22	49	ii. T.
	23	4	i. *o.*
3	1	25	i. E.
	20	15	i. *t.*
	22	26	i. T.
4	15	0	ii. *o.*
	17	30	i. *o.*
	17	42	ii. E.
	19	53	i. E.
5	14	41	i. *t.*
	16	52	i. T.
6	5	30	iii. *t.*
	6	49	iii. T.
	9	42	ii. *t.*
	11	56	i. *o.*
	14	22	i. E.
7	9	7	i. *t.*
	11	18	i. T.
8	4	8	ii. *o.*
	6	22	i. *o.*
	7	1	ii. E.
	8	50	i. E.
9	3	34	i. *t.*
	5	45	i. T.
	19	18	iii. *o.*
	22	9	iii. E.
	22	48	ii. *t.*
10	0	48	i. *o.*
	1	3	ii. T.
	3	19	i. E.
	22	0	i. *t.*
11	0	11	i. T.

May (con.).

d	h	m	
11	17	16	ii. *o.*
	19	14	i. *o.*
	20	19	ii. E.
	21	47	i. E.
12	16	26	i. *t.*
	18	38	i. T.
13	8	47	iii. *t.*
	10	12	iii. T.
	11	55	ii. *t.*
	13	41	i. *o.*
	14	10	ii. T.
	16	16	i. E.
14	10	52	i. *t.*
	13	4	i. T.
15	6	25	ii. *o.*
	8	7	i. *o.*
	9	38	ii. E.
	10	44	i. E.
16	5	19	i. *t.*
	7	30	i. T.
	22	34	iii. *o.*
17	0	2	iii. O.
	0	35	iii. *e.*
	1	2	ii. *t.*
	2	6	iii. E.
	2	33	i. *o.*
	3	18	ii. T.
	5	13	i. E.
	23	45	i. *t.*
18	1	56	i. T.
	19	33	ii. *o.*
	20	59	i. *o.*
	22	56	ii. E.
	23	41	i. E.
19	18	12	i. *t.*
	20	23	i. T.
20	12	5	iii. *t.*
	13	36	iii. T.
	14	10	ii. *t.*
	15	25	i. *o.*
	16	26	ii. T.
	18	10	i. E.
21	12	38	i. *t.*
	14	49	i. T.
22	8	42	ii. *o.*
	9	52	i. *o.*
	12	15	ii. E.
	12	38	i. E.
23	7	5	i. *t.*
	9	16	i. T.
24	1	54	iii. *o.*
	3	17	ii. *t.*
	3	27	iii. O.
	4	18	i. *o.*
	4	34	iii. *e.*
	5	34	ii. T.
	6	5	iii. E.
	7	7	i. E.
25	1	31	i. *t.*
	3	43	i. T.
	21	52	ii. *o.*
	22	44	i. *o.*
26	1	33	ii. E.
	1	35	i. E.

May (co

d	h	m	
26	19	58	i
	22	9	i
27	15	27	i
	16	26	i
	17	3	i
	17	11	i
	18	43	i
	20	4	i
28	14	24	i
	16	36	i
29	11	2	i
	11	36	i
	14	32	i
	14	52	i
30	8	51	i
	11	3	i
31	5	17	i
	5	34	i
	6	4	i
	6	55	i
	7	52	i
	8	33	i
	9	1	i
	10	3	i

June.

d	h	m	
1	3	18	i
	5	30	i
2	0	13	i
	0	31	i
	3	29	i
	4	10	i
	21	45	i
	23	57	i
3	18	44	i
	18	52	i
	18	57	i
	20	32	i
	21	2	i
	21	58	i
4	16	12	i
	18	23	i
5	13	24	i
	13	24	i
	16	27	i
	17	29	i
6	10	39	i
	12	51	i
7	7	51	i
	7	54	i
	8	44	i
	10	12	i
	10	27	i
	10	55	i
	12	32	i
	14	2	i
8	5	6	i
	7	18	i
9	2	18	i
	2	35	i
	5	24	i
	6	47	i
	23	33	i

June (con.).

d	h	m	
10	1	45	i. T.
	20	45	i. *o.*
	21	4	ii. *t.*
	22	21	iii. *t.*
	23	23	ii. T.
	23	52	i. E.
11	0	6	iii. T.
	18	0	i. *t.*
	20	12	i. T.
12	15	12	i. *o.*
	15	47	ii. *o.*
	18	21	i. E.
	20	7	ii. E.
13	12	27	i. *t.*
	14	39	i. T.
14	9	39	i. *o.*
	10	15	ii. *t.*
	12	15	iii. *o.*
	12	34	ii. T.
	12	50	i. E.
	14	3	iii. O.
	16	31	iii. *e.*
	18	0	iii. E.
15	6	55	i. *t.*
	9	7	i. T.
16	4	6	i. *o.*
	5	0	ii. *o.*
	7	18	i. E.
	9	25	ii. E.
17	1	22	i. T.
	3	34	i. T.
	22	34	i. *o.*
	23	26	ii. *t.*
18	1	46	ii. T.
	1	47	i. E.
	1	54	iii. *t.*
	3	45	iii. T.
	19	49	i. *t.*
	22	2	i. T.
19	17	1	i. *o.*
	18	13	ii. *o.*
	20	16	i. E.
	22	44	ii. E.
20	14	17	i. *t.*
	16	29	i. T.
21	11	28	i. *o.*
	12	38	ii. *t.*
	14	44	i. E.
	14	58	ii. T.
	15	50	iii. *o.*
	17	43	iii. O.
	20	30	iii. *e.*
	21	58	iii. E.
22	8	45	i. *t.*
	10	57	i. T.
23	5	56	i. *o.*
	7	27	ii. *o.*
	9	13	i. E.
	12	2	ii. E.
24	3	12	i. *t.*
	5	25	i. T.
25	0	24	i. *o.*
	1	51	ii. *t.*
	3	41	i. E.

June (con.).

d	h	m	
25	4	11	ii. T.
	5	32	iii. *t.*
	7	27	iii. T.
	21	40	i. *t.*
	23	52	i. T.
26	18	51	i. *o.*
	20	41	ii. *o.*
	22	10	i. E.
	23	2	ii. O.
	23	6	ii. *e.*
27	1	21	ii. E.
	16	8	i *t.*
	18	20	i. T.
28	13	19	i. *o.*
	15	4	ii. *t.*
	16	39	i. E.
	17	25	ii. T.
	19	29	iii. *o.*
	21	26	iii. O.
29	0	29	iii. *e.*
	1	57	iii. E.
	10	36	i. *t.*
	12	48	i. T.
30	7	46	i. *o.*
	9	56	ii. *o.*
	11	7	i. E.
	12	17	ii. O.
	12	24	ii. *e.*
	14	39	ii. E.

July.

d	h	m	
1	5	3	i. *t.*
	7	16	i. T.
2	2	14	i. *o.*
	4	18	ii. *t.*
	5	36	i. E.
	6	39	ii. T.
	9	16	iii. *t.*
	11	14	iii. T.
	23	32	i. *t.*
3	1	44	i. T.
	20	42	i. *o.*
	23	11	ii. *o.*
4	0	5	i. E.
	1	33	ii. O.
	1	43	ii. *e.*
	3	59	ii. E.
	18	0	i. *t.*
	20	12	i. T.
5	15	10	i. *o.*
	17	32	ii. *t.*
	18	33	i. E.
	19	54	ii. T.
	23	14	iii. *o.*
6	1	14	iii. O.
	4	28	iii. *e.*
	5	55	iii. E.
	12	28	i. *t.*
	14	40	i. T.
7	9	38	i. *o.*
	12	27	ii. *o.*
	13	2	i. E.
	14	49	ii. O.

July (con.).

d	h	m	
7	15	2	ii. *e.*
	17	17	ii. E.
8	6	56	i. *t.*
	9	9	i. T.
9	4	6	i. *o.*
	6	47	ii. *t.*
	7	31	i. E.
	9	9	ii. T.
	13	3	iii. *t.*
	15	4	iii. T.
10	1	24	i. *t.*
	3	37	i. T.
	22	35	i. *o.*
11	1	44	ii. *o.*
	1	59	i. E.
	4	6	ii. O.
	4	21	ii. *e.*
	6	36	ii. E.
	19	53	i. *t.*
	22	6	i. T.
12	17	3	i. *o.*
	20	3	ii. *t.*
	20	28	i. E.
	22	25	ii. T.
13	3	4	iii. *o.*
	5	6	iii. O.
	8	27	iii. *e.*
	9	55	iii. E.
	14	21	i. *t.*
	16	34	i. T.
14	11	31	i. *o.*
	14	57	i. E.
	15	1	ii. *o.*
	17	24	ii. O.
	17	39	ii. *e.*
	19	54	ii. E.
15	8	50	i. *t.*
	11	2	i. T.
16	6	0	i. *o.*
	9	19	ii. *t.*
	9	25	i. E.
	11	42	ii. T.
	16	55	iii. *t.*
	18	58	iii. T.
17	3	18	i. *t.*
	5	31	i. T.
18	0	28	i. *o.*
	3	54	i. E.
	4	19	ii. *o.*
	6	42	ii. O.
	6	58	ii. *e.*
	9	13	ii. E.
	21	47	i. *t.*
19	0	0	i. T.
	18	57	i. *o.*
	22	23	i. E.
	22	36	ii. *t.*
20	0	59	ii. T.
	6	58	iii. *o.*
	9	2	iii. O.
	12	26	iii. *e.*
	13	53	iii. E.
	16	15	i. *t.*
	18	28	i. T.

July (con.).

d	h	m	
21	13	25	i. *o.*
	16	51	i. E.
	17	37	ii. *o.*
	20	0	ii. O.
	20	16	ii. *e.*
	22	31	ii. E.
22	10	44	i. *t.*
	12	57	i. T.
23	7	54	i. *o.*
	11	20	i. E.
	11	53	ii. *t.*
	14	16	ii. T.
	20	51	iii. *t.*
	22	56	iii. T.
24	5	13	i. *t.*
	7	26	i. T.
25	2	23	i. *o.*
	5	49	i. E.
	6	55	ii. *o.*
	9	19	ii. O.
	9	35	ii. *e.*
	11	50	ii. E.
	23	42	i. *t.*
26	1	55	i. T.
	20	51	i. *o.*
27	0	17	i. E.
	1	10	ii. *t.*
	3	34	ii. T.
	10	57	iii. *o.*
	13	2	iii. O.
	16	26	iii. *e.*
	17	52	iii. E.
	18	11	i. *t.*
	20	24	i. T.
28	15	20	i. *o.*
	18	46	i. E.
	20	14	ii. *o.*
	22	38	ii. O.
	22	53	ii. *e.*
29	1	9	ii. E.
	12	39	i. *t.*
	14	52	i. T.
30	9	49	i. *o.*
	13	15	i. E.
	14	29	ii. *t.*
	16	53	ii. T.
31	0	52	iii. *t.*
	2	58	iii. T.
	7	8	i. *t.*
	9	21	i. T.

August.

d	h	m	
1	4	18	i. *o.*
	7	44	i. E.
	9	33	ii. *o.*
	11	57	ii. O.
	12	12	ii. *e.*
	14	28	ii. E.
2	1	37	i. *t.*
	3	50	i. T.
	22	47	i. *o.*
3	2	12	i. E.
	3	47	ii. *t.*
	6	11	ii. T.

Aug. (con.).

d	h	m	
3	14	59	iii. *o.*
	17	6	iii. O.
	20	7	i. *t.*
	20	25	iii. *e.*
	21	51	iii. E.
	22	20	i. T.
4	17	16	i. *o.*
	20	41	i. E.
	22	53	ii. *o.*
5	1	18	ii. O.
	1	30	ii. *e.*
	3	46	ii. E.
	14	36	i. *t.*
	16	49	i. T.
6	11	45	i. *o.*
	15	10	i. E.
	17	7	ii. *t.*
	19	31	ii. T.
7	4	56	iii. *t.*
	7	3	iii. T.
	9	5	i. *t.*
	11	18	i. T.
8	6	14	i. *o.*
	9	38	i. E.
	12	14	ii. *o.*
	14	38	ii. O.
	14	49	ii. *e.*
	17	4	ii. E.
9	3	34	i. *t.*
	5	47	i. T.
10	0	43	i. *o.*
	4	7	i. E.
	6	26	ii. *t.*
	8	51	ii. T.
	19	5	iii. *o.*
	21	12	iii. O.
	22	3	i. *t.*
11	0	17	i. T.
	0	24	iii. *e.*
	1	50	iii. E.
	19	12	i. *o.*
	22	36	i. E.
12	1	34	ii. *o.*
	3	59	ii. O.
	4	7	ii. *e.*
	6	22	ii. E.
	16	33	i. *t.*
	18	46	i. T.
13	13	41	i. *o.*
	17	4	i. E.
	19	46	ii. *t.*
	22	11	ii. T.
14	9	4	iii. *t.*
	11	2	i. *t.*
	11	12	iii. T.
	13	15	i. T.
15	8	11	i. *o.*
	11	33	i. E.
	14	55	ii. *o.*
	17	20	ii. O.
	17	26	ii. *e.*
	19	41	ii. E.
16	5	31	i. *t.*
	7	45	i. T.

Aug. (

d	h	m
17	2	4
	6	
	9	
	11	3
	23	1
18	0	
	1	2
	2	1
	4	2
	5	4
	21	1
19	0	3
	4	1
	6	4
	6	4
	8	5
	18	3
	20	4
20	15	3
	18	5
	22	2
21	0	5
	13	
	13	1
	15	1
	15	24
22	10	
	13	2
	17	3
	22	1
23	7	3
	9	4
24	4	3
	7	5
	11	4
	14	1
25	1	5
	3	2
	4	1
	5	3
	8	2
	9	4
	23	
26	2	2
	7	
	11	3
	20	2
	22	4
27	17	3
	20	5
28	1	1
	3	3
	14	5
	17	1
	17	3
	19	3
29	12	
	15	2
	20	2
30	0	5
	9	2
	11	4
31	6	3
	9	5

Aug. (con.).

d	h	m	
31	14	33	ii. *t*.
	16	58	ii. T.

September.

d	h	m	
1	3	58	i. *t*.
	6	11	i. T.
	7	42	iii. *o*.
	9	50	iii. O.
	12	21	iii. *e*.
	13	46	iii. E.
2	1	7	i. *o*.
	4	20	i. E.
	9	45	ii. *o*.
	14	12	ii. E.
	22	28	i. *t*.
3	0	41	i. T.
	19	36	i. *o*.
	22	49	i. E.
4	3	56	ii. *t*.
	6	20	ii. T.
	16	58	i. *t*.
	19	11	i. T.
	21	46	iii. *t*.
	23	54	iii. T.
5	14	6	i. *o*.
	17	17	i. E.
	23	8	ii. *o*.
6	3	30	ii. E.
	11	28	i. *t*.
	13	41	i. T.
7	8	36	i. *o*.
	11	46	i. E.
	17	18	ii. *t*.
	19	43	ii. T.
8	5	57	i. *t*.
	8	11	i. T.
	12	0	iii. *o*.
	14	8	iii. O.
	16	21	iii. *e*.
	17	45	iii. E.
9	3	6	i. *o*.
	6	15	i. E.
	12	31	ii. *o*.
	16	48	ii. E.
10	0	27	i. *t*.
	2	41	i. T.
	21	36	i. *o*.

Sept. (con.).

d	h	m	
11	0	44	i. E.
	6	41	ii. *t*.
	9	6	ii. T.
	18	57	i. *t*.
	21	11	i. T.
12	2	4	iii. *t*.
	4	12	iii. T.
	16	6	i. *o*.
	19	12	i. E.
13	1	54	ii. *o*.
	6	7	ii. E.
	13	27	i. *t*.
	15	40	i. T.
14	10	36	i. *o*.
	13	41	i. E.
	20	4	ii. *t*.
	22	29	ii. T.
15	7	57	i. *t*.
	10	10	i. T.
	16	20	iii. *o*.
	18	27	iii. O.
	20	20	iii. *e*.
	21	45	iii. E.
16	5	6	i. *o*.
	8	10	i. E.
	15	17	ii. *o*.
	19	24	ii. E.
17	2	27	i. *t*.
	4	40	i. T.
	23	36	i. *o*.
18	2	38	i. E.
	9	28	ii. *t*.
	11	53	ii. T.
	20	57	i. *t*.
	23	11	i. T.
19	6	25	iii. *t*.
	8	32	iii. T.
	18	6	i. *o*.
	21	7	i. E.
20	4	41	ii. *o*.
	8	42	ii. E.
	15	27	i. *t*.
	17	41	i. T.
21	12	36	i. *o*.
	15	36	i. E.
	22	52	ii. *t*.
22	1	16	ii. T.
	9	57	i. *t*.

Sept. (con.).

d	h	m	
22	12	11	i. T.
	20	42	iii. *o*.
	22	48	iii. O.
23	0	19	iii. *e*.
	1	43	iii. E.
	7	6	i. *o*.
	10	4	i. E.
	18	4	ii. *o*.
	22	0	ii. E.
24	4	27	i. *t*.
	6	41	i. T.
25	1	36	i. *o*.
	4	33	i. E.
	12	16	ii. *t*.
	14	40	ii. T.
	22	58	i. *t*.
26	1	11	i. T.
	10	48	iii. *t*.
	12	54	iii. T.
	20	6	i. *o*.
	23	2	i. E.
27	7	28	ii. *o*.
	11	18	ii. E.
	17	28	i. *t*.
	19	41	i. T.
28	14	36	i. *o*.
	17	30	i. E.
29	1	40	ii. *t*.
	4	5	ii. T.
	11	58	i. *t*.
	14	11	i. T.
30	1	6	iii. *o*.
	3	11	iii. O.
	4	18	iii. *e*.
	5	42	iii. E.
	9	7	i. *o*.
	11	59	i. E.
	20	52	ii. *o*.

October.

d	h	m	
1	0	36	ii. E.
	6	28	i. *t*.
	8	41	i. T.
2	3	37	i. *o*.
	6	28	i. E.
	15	4	ii. *t*.
	17	29	ii. T.

Oct. (con.).

d	h	m	
3	0	58	i. *t*.
	3	11	i. T.
	15	13	iii. *t*.
	17	18	iii. T.
	22	7	i. *o*.
4	0	56	i. E.
	10	16	ii. *o*.
	13	53	ii. E.
	19	28	i. *t*.
	21	42	i. T.
5	16	37	i. *o*.
	19	25	i. E.
6	4	28	ii. *t*.
	6	53	ii. T.
	13	59	i. *t*.
	16	12	i. T.
7	5	31	iii. *o*.
	7	35	iii. O.
	8	16	iii. *e*.
	9	41	iii. E.
	11	8	i. *o*.
	13	54	i. E.
	23	40	ii. *o*.
8	3	11	ii. E.
	8	29	i. *t*.
	10	42	i. T.
9	53	8	i. *o*.
	8	22	i. E.
	17	53	ii. *t*.
	20	18	ii. T.
10	2	59	i. *t*.
	5	12	i. T.
	19	39	iii. *t*.
	21	43	iii. T.
11	0	8	i. *o*.
	2	51	i. E.
	13	5	ii. O.
	16	29	ii. E.
	21	29	i. *t*.
	23	43	i. T.
12	18	39	i. *o*.
	21	20	i. E.
13	7	18	ii. *t*.
	9	43	ii. T.
	16	0	i. *t*.
	18	13	i. T.
14	9	56	iii. *o*.
	12	1	iii. O.

Oct. (con.).

d	h	m	
14	12	15	iii. *e*.
	13	9	i. *o*.
	13	39	iii. E.
	15	48	i. E.
15	2	29	ii. *o*.
	5	46	ii. E.
	10	30	i. *t*.
	12	43	i. T.

December.

d	h	m	
11	0	3	ii. *e*.
	2	37	i. *t*.
	3	19	ii. O.
	4	50	i. T.
	23	24	i. *e*.
12	2	3	i. O.
	19	27	ii. *t*.
	21	7	i. *t*.
	21	51	ii. T.
	23	20	i. T.
13	17	52	i. *e*.
	20	33	i. O.
14	11	48	iii. *t*.
	13	19	ii. *e*.
	13	41	iii. T.
	15	37	i. *t*.
	16	43	ii. O.
	17	50	i. T.
15	12	21	i. *e*.
	15	4	i. O.
16	8	51	ii. *t*.
	10	7	i. *t*.
	11	16	ii. T.
	12	20	i. T.
17	6	49	i. *e*.
	9	34	i. O.
18	0	0	iii. *e*.
	1	27	iii. E.
	2	9	iii. *o*.
	2	36	ii. *e*.
	4	2	iii. O.
	4	37	i. *t*.
	6	6	ii. O.
	6	50	i. T.
19	1	18	i. *e*.
	4	4	i. O.
	22	16	ii. *t*.
	23	7	i. *t*.

Dec. (c

d	h	m
20	0	41
	1	20
	19	46
	22	34
21	15	52
	16	12
	17	37
	18	4
	19	29
	19	50
22	14	15
	17	4
23	11	40
	12	7
	14	4
	14	20
24	8	43
	11	34
25	3	59
	5	9
	5	26
	6	35
	6	37
	8	26
	8	50
	8	52
26	3	11
	6	4
27	1	5
	1	7
	3	20
	3	29
	21	40
28	0	34
	18	26
	19	37
	20	36
	21	49
	22	14
	22	27
29	16	8
	19	4
30	14	7
	14	29
	16	19
	16	53
31	10	37
	13	34

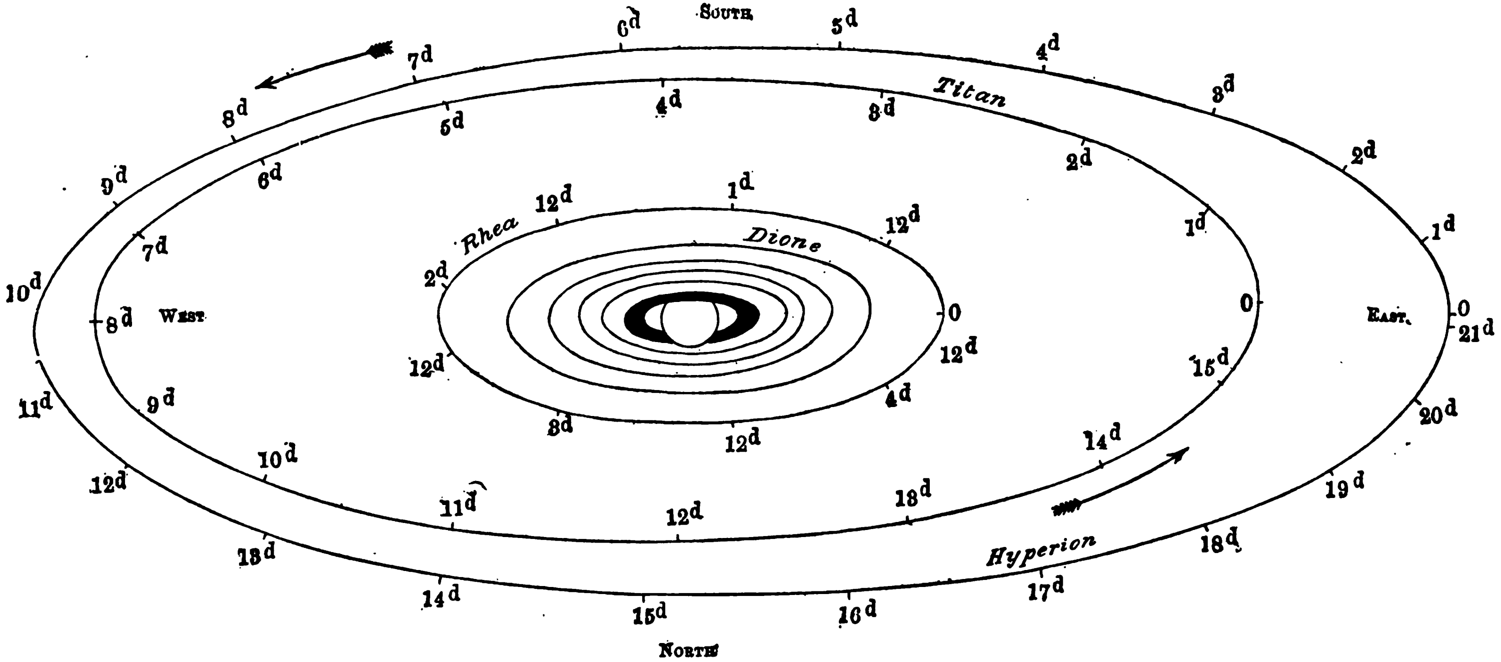
The satellites of Jupiter will be invisible from October 16 until December 11, Jupiter being to the Sun.

Satellite I. *disappears* at eclipse on the *p* side in January, February, March, April, and Decemb *reappears* on the *f* side from May to October.

Satellites II. & III. *disappear* and *reappear* at eclipse on the *p* side in January, February, April, and December. On the *f* side from May to October.

There is no eclipse of Satellite IV. throughout the year.

APPARENT ORBITS OF THE SEVEN INNER SATELLITES OF SATURN, as seen in an Inverting Telescope.



The arrows in the diagram show the direction of the motion of the Satellites. The figures indicate the interval from the time of last East Elongation.

SATELLITES OF SATURN.

East Elongations.

MIMAS.

Feb.		March.		April.		May.		June.		July.		Aug.		Sept.		Oc	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	
1	6·5	1	13·1	1	15·5	1	19·1	1	21·4	1	2·4	1	4·8	1	7·3	1	1
2	5·1	2	11·7	2	14·1	2	17·8	2	20·0	2	1·0	2	3·4	2	5·9	2	
3	3·7	3	10·3	3	12·7	3	16·4	3	18·6	2	23·7	3	2·0	3	4·5	3	
4	2·3	4	9·0	4	11·3	4	15·0	4	17·3	3	22·3	4	0·6	4	3·1	4	
5	0·9	5	7·6	5	9·9	5	13·6	5	15·9	4	20·9	4	23·3	5	1·8	5	
5	23·6	6	6·2	6	8·6	6	12·2	6	14·5	5	19·5	5	21·9	6	0·4	6	
6	22·2	7	4·8	7	7·2	7	10·9	7	13·1	6	18·1	6	20·5	6	23·0	7	
7	20·8	8	3·4	8	5·8	8	9·5	8	11·7	7	16·8	7	19·1	7	21·6	8	
8	19·4	9	2·0	9	4·4	9	8·1	9	10·3	8	15·4	8	17·7	8	20·2	9	
9	18·0	10	0·6	10	3·0	10	6·7	10	8·9	9	14·0	9	16·3	9	18·9	9	2
10	16·7	10	23·3	11	1·6	11	5·3	11	7·6	10	12·6	10	15·0	10	17·5	10	2
11	15·3	11	21·9	12	0·3	12	3·9	12	6·2	11	11·2	11	13·6	11	16·1	11	2
12	13·9	12	20·5	12	22·9	13	2·5	13	4·8	12	9·8	12	12·2	12	14·7	12	1
13	12·5	13	19·1	13	21·5	14	1·1	14	3·4	13	8·4	13	10·8	13	13·4	13	1
14	11·2	14	17·7	14	20·1	14	23·8	15	2·0	14	7·1	14	9·5	14	12·0	14	1
15	9·8	15	16·4	15	18·7	15	22·4	16	0·6	15	5·7	15	8·1	15	10·6	15	1
16	8·4	16	15·0	16	17·3	16	21·0	16	23·2	16	4·3	16	6·7	16	9·2	16	1
17	7·0	17	13·6	17	16·0	17	19·6	17	21·9	17	2·9	17	5·3	17	7·8	17	1
18	5·7	18	12·2	18	14·6	18	18·2	18	20·5	18	1·5	18	4·0	18	6·5	18	1
19	4·3	19	10·8	19	13·2	19	16·8	19	19·1	19	0·2	19	2·6	19	5·1	19	
20	2·9	20	9·5	20	11·8	20	15·5	20	17·7	19	22·8	20	1·2	20	3·7	20	
21	1·5	21	8·1	21	10·4	21	14·1	21	16·3	20	21·4	20	23·8	21	2·3	21	
22	0·1	22	6·7	22	9·0	22	12·7	22	14·9	21	20·0	21	22·4	22	1·0	22	
22	22·8	23	5·3	23	7·6	23	11·3	23	13·5	22	18·6	22	21·1	22	23·6	23	
23	21·4	24	3·9	24	6·2	24	9·9	24	12·1	23	17·2	23	19·7	23	22·2	24	
24	20·0	25	2·5	25	4·9	25	8·5	25	10·8	24	15·9	24	18·3	24	20·8	25	
25	18·6	26	1·2	26	3·5	26	7·1	26	9·4	25	14·5	25	16·9	25	19·5	25	2
26	17·2	26	23·8	27	2·1	27	5·7	27	8·0	26	13·1	26	15·5	26	18·1	26	2
27	15·9	27	22·4	28	0·7	28	4·3	28	6·6	27	11·7	27	14·2	27	16·7	27	2
28	14·5	28	21·0	28	23·3	29	3·0	29	5·2	28	10·3	28	12·8	28	15·3	28	1
		29	19·6	29	21·9	30	1·6	30	3·8	29	8·9	29	11·4	29	14·0	29	1
		30	18·2	30	20·5	31	0·2			30	7·6	30	10·0	30	12·6	30	1
		31	16·9			31	22·8			31	6·2	31	8·6			31	1

ENCELADUS.

Feb.		March.		April.		May.		June.		July.		Aug.		Sept.		O	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	
1	17·2	1	3·1	1	15·4	1	18·8	2	6·9	1	1·3	1	13·6	2	2·0	2	
3	2·1	2	11·9	3	0·3	3	3·7	3	15·8	2	10·2	2	22·5	3	10·9	3	1
4	11·0	3	20·8	4	9·2	4	12·5	5	0·7	3	19·1	4	7·4	4	19·8	4	2
5	19·9	5	5·7	5	18·1	5	21·4	6	9·6	5	4·0	5	16·3	6	4·7	6	
7	4·8	6	14·6	7	3·0	7	6·3	7	18·4	6	12·8	7	1·1	7	13·6	7	1
8	13·7	7	23·5	8	11·8	8	15·2	9	3·3	7	21·7	8	10·0	8	22·5	9	
9	22·6	9	8·4	9	20·7	10	0·0	10	12·2	9	6·6	9	18·9	10	7·4	10	1
11	7·5	10	17·3	11	5·6	11	8·9	11	21·1	10	15·5	11	3·8	11	16·3	11	1
12	16·4	12	2·2	12	14·5	12	17·8	13	5·9	12	0·4	12	12·6	13	1·1	13	
14	1·3	13	11·0	13	23·4	14	2·7	14	14·8	13	9·2	13	21·5	14	10·0	14	1
15	10·1	14	19·9	15	8·2	15	11·6	15	23·7	14	18·1	15	6·4	15	18·9	15	2
16	19·0	16	4·8	16	17·1	16	20·4	17	8·6	16	3·0	16	15·3	17	3·8	17	
18	3·9	17	13·7	18	2·0	18	5·3	18	17·4	17	11·9	18	0·2	18	12·7	18	1
19	12·8	18	22·6	19	10·9	19	14·2	20	2·3	18	20·8	19	9·1	19	21·6	20	
20	21·7	20	7·5	20	19·8	20	23·0	21	11·2	20	5·6	20	18·0	21	6·5	21	1
22	6·6	21	16·4	22	4·6	22	7·9	22	20·1	21	14·5	22	2·9	22	15·4	22	1
23	15·5	23	1·2	23	13·5	23	16·8	24	4·9	22	23·4	23	11·7	24	0·3	24	
25	0·4	24	10·1	24	22·4	25	1·7	25	13·8	24	8·3	24	20·6	25	9·2	25	1
26	9·3	25	19·0	26	7·3	26	10·6	26	22·7	25	17·2	26	5·5	26	18·1		
27	18·2	27	3·9	27	16·2	27	19·5	28	7·6	27	2·0	27	14·4	28	3·0		
		28	12·7	29	1·0	29	4·3	29	16·5	28	10·9	28	23·3	29	11·9		
		29	21·6	30	9·9	30	13·2			29	19·8	30	8·2	30	20·8		
		31	6·5			31	22·1			31	4·7	31	17·1				

East Elongations.

TETHYS.

Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.
d h	d h	d h	d h	d h	d h	d h	d h	d h
1 18·1	2 1·9	1 6·9	1 11·7	2 13·6	2 18·2	1 23·0	1 3·9	1 9·1
3 15·5	3 23·2	3 4·2	3 9·0	4 10·9	4 15·5	3 20·3	3 1·3	3 6·5
5 12·8	5 20·6	5 1·5	5 6·3	6 8·2	6 12·8	5 17·6	4 22·6	5 3·8
7 10·1	7 17·9	6 22·8	7 3·5	8 5·5	8 10·1	7 14·9	6 19·9	7 1·1
9 7·4	9 15·2	8 20·1	9 0·8	10 2·8	10 7·4	9 12·2	8 17·2	8 22·4
11 4·8	11 12·5	10 17·4	10 22·1	12 0·0	12 4·7	11 9·5	10 14·6	10 19·8
13 2·1	13 9·8	12 14·7	12 19·4	13 21·3	14 2·0	13 6·8	12 11·9	12 17·1
14 23·4	15 7·1	14 12·0	14 16·7	15 18·6	15 23·3	15 4·1	14 9·2	14 14·4
16 20·7	17 4·4	16 9·3	16 14·0	17 15·9	17 20·6	17 1·4	16 6·5	16 11·7
18 18·1	19 1·7	18 6·6	18 11·3	19 13·2	19 17·9	18 22·7	18 3·9	18 19·1
20 15·4	20 23·0	20 3·9	20 8·6	21 10·5	21 15·2	20 20·0	20 1·2	20 6·4
22 12·7	22 20·3	22 1·2	22 5·9	23 7·8	23 12·5	22 17·4	21 22·5	
24 10·0	24 17·6	23 22·5	24 3·2	25 5·1	25 9·8	24 14·7	23 19·8	
26 7·3	26 14·9	25 19·8	26 0·5	27 2·3	27 7·1	26 12·0	25 17·2	
28 4·6	28 12·3	27 17·1	27 21·7	28 23·6	29 4·4	28 9·3	27 14·5	
	30 9·6	29 14·4	29 19·0	30 20·9	31 1·7	30 6·6	29 11·8	
			31 16·3					

DIONE.

Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.
d h	d h	d h	d h	d h	d h	d h	d h	d h
1 8·8	3 11·7	2 14·3	2 16·6	1 18·8	1 20·9	3 16·9	2 19·5	2 22·4
4 2·5	6 5·4	5 8·0	5 10·3	4 12·4	4 14·6	6 10·6	5 13·2	5 16·1
6 20·2	8 23·1	8 1·6	8 3·9	7 6·1	7 8·2	9 4·3	8 6·9	8 9·8
9 13·9	11 16·8	10 19·3	10 21·6	9 23·7	10 1·9	11 22·0	11 0·6	11 3·5
12 7·7	14 10·5	13 13·0	13 15·2	12 17·4	12 19·5	14 15·6	13 18·3	13 21·3
15 1·4	17 4·2	16 6·6	16 8·9	15 11·0	15 13·2	17 9·3	16 12·0	16 15·0
17 19·1	19 21·8	19 0·3	19 2·5	18 4·7	18 6·9	20 3·0	19 5·7	19 8·8
20 12·8	22 15·5	21 18·0	21 20·2	20 22·3	21 0·6	22 20·7	21 23·4	22 2·5
23 6·6	25 9·2	24 11·6	24 13·9	23 16·0	23 18·2	25 14·4	24 17·1	24 20·2
26 0·3	28 2·9	27 5·3	27 7·5	26 9·6	26 11·9	28 8·1	27 10·8	27 14·0
28 18·0	30 20·6	29 23·0	30 1·1	29 3·3	29 5·6	31 1·8	30 4·6	30 7·7
					31 23·3			

RHEA.

Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.
d h	d h	d h	d h	d h	d h	d h	d h	d h
11 22·5	2 0·6	2 15·7	4 6·3	4 20·6	1 22·5	2 13·0	3 4·0	4 19·5
16 11·1	6 13·0	7 4·1	8 18·6	9 8·9	6 10·8	7 1·4	7 16·4	9 8·0
20 23·6	11 1·5	11 16·5	13 7·0	13 21·2	10 23·2	11 13·8	12 4·9	13 20·5
25 12·1	15 14·0	16 4·9	17 19·3	18 9·5	15 11·5	16 2·2	16 17·4	
	20 2·4	20 17·2	22 7·6	22 21·9	19 23·9	20 14·6	21 6·0	
	24 14·9	25 5·6	26 19·9	27 10·2	24 12·2	25 3·1	25 18·5	
	29 3·3	29 17·9	31 8·3		29 0·6	29 15·5	30 7·0	

TITAN.

Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.
d h	d h	d h	d h	d h	d h	d h	d h	d h
2 16·1	6 15·9	7 13·8	9 10·1	10 5·0	11 23·8	12 19·9	13 18·0	15 17·7
18 16·2	22 15·2	23 12·1	25 7·6	26 2·2	27 21·7	28 18·8	29 17·7	31 18·0

East Elongations.

HYPERION.

Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.
d h	d h	d h	d h	d h	d h	d h	d h	d h
4 17·8	19 9·8	9 15·1	21 21·6	11 23·5	3 1·4	14 7·2	4 11·5	17 0·2
26 2·4		30 18·7			24 3·8		25 17·3	

IAPETUS.

		d h		d h		d h		d h
E. Elongation	Jan.	31 6·2,	Apr.	21 5·3,	July	8 7·9,	Sept.	25 13·4
Inf. Conjunction ...	Feb.	19 16·6,	May	10 2·2,	July	27 4·1,	Oct.	15 0·2
W. Elongation......	Mar.	12 10·8,	May	30 7·0,	Aug.	16 14·4,	Nov.	4 23·8
Sup. Conjunction...	Apr.	2 2·9,	June	19 11·8,	Sept.	6 4·8.		

Apparent Elements of Saturn's Rings.

Greenwich Mean Noon.	Position-angle of Minor Axis.	Outer Ring.		Latitude above Plane of Ring.	
		Maj. Axis.	Min. Axis.	Earth.	Sun.
	° ′	″	″	° ′	° ′
Jan. 15	5 9·1	35·64	16·06	26 46·7 N.	26 39·2 N.
Feb. 4	5 21·2	36·44	16·39	26 44·0 N.	26 41·3 N.
24	5 30·1	37·49	16·82	26 39·8 N.	26 43·0 N.
Mar. 16	5 35 2	38·73	17·34	26 36·0 N.	26 44·5 N.
April 5	5 36·5	40·04	17·90	26 33·8 N.	26 45·8 N.
25	5 33·9	41·25	18·44	26 33·6 N.	26 46·8 N.
May 15	5 27·9	42·17	18·88	26 35·5 N.	26 47·7 N.
June 4	5 19·5	42·64	19·12	26 38·5 N.	26 48·4 N.
24	5 10·2	42·56	19·12	26 41·9 N.	26 49·1 N.
July 14	5 1·8	41·95	18·88	26 45·0 N.	26 49·5 N.
Aug. 3	4 56·2	40·94	18·46	26 48·4 N.	26 49·6 N.
23	4 54·5	39·70	17·94	26 52·0 N.	26 49·6 N.
Sept. 12	4 57·2	38·40	17·40	26 56·1 N.	26 49·3 N.
Oct. 2	5 4·0	37·20	16·89	26 59·9 N.	26 48·9 N.
22	5 14·2	36·20	16·46	27 2·2 N.	26 48·4 N.
Nov. 11	5 26·6	35·46	16·11	27 1·8 N.	26 47·6 N.
Dec. 1	5 40·2	35·03	15·88	26 57·9 N.	26 46·6 N.
21	5 53·7	34·92	15·76	26 50·0 N.	26 45·5 N.

SATELLITES OF URANUS.

North Elongations.

Ariel.

March.	April.	May.	June.	July.	August.	September.
d h	d h	d h	d h	d h	d h	d h
	1 12·7	1 19·0	1 1·3	1 7·7	3 2·4	2 8·3
	4 1·2	4 7·5	3 13·9	3 20·3	5 14·9	4 20·8
	6 13·7	6 20·0	6 2·4	6 8·8	8 3·4	7 9·3
	9 2·2	9 8·5	8 14·9	8 21·3	10 15·9	9 21·8
	11 14·7	11 21·1	11 3·5	11 9·9	13 4·4	12 10·3
	14 3·2	14 9·6	13 16·0	13 22·4	15 16·9	14 22·8
	16 15·7	16 22·1	16 4·5	16 10·9	18 5·4	17 11·2
	19 4·3	19 10·7	18 17·1	18 23·4	20 17·9	19 23·7
	21 16·8	21 23·2	21 5·6	21 11·9	23 6·4	22 12·2
	24 5·3	24 11·7	23 18·1	24 0·4	25 18·9	25 0·5
27 11·7	26 17·9	27 0·3	26 6·7	26 12·9	28 7·4	27 13·0
30 0·2	29 6·4	29 12·8	28 19·2	29 1·4	30 19·9	30 1·5
				31 13·9		

Umbriel.

March.	April.	May.	June.	July.	August.	September.
d h	d h	d h	d h	d h	d h	d h
	1 16·3	4 20·8	2 22·0	1 22·9	4 3·0	
	5 19·8	9 0·3	7 1·5	6 2·4	8 6·5	6 6·7
	9 23·4	13 4·0	11 5·0	10 6·0	12 10·0	10 10·1
	14 2·9	17 7·5	15 8·7	14 9·5	16 13·5	14 13·5
16 2·3	18 6·5	21 11·1	19 22·2	18 13·0	20 17·0	18 27·0
20 5·8	22 10·0	25 14·6	23 15·8	22 16·5	24 20·5	22 20·3
24 9·3	26 13·6	29 18·3	27 19·3	26 20·1	28 23·9	26 23·7
28 12·8	30 17·1			30 23·6		
						October.
						1 3·0

Titania.

February.	March.	April.	May.	June.	July.	August.	September.
d h	d h	d h	d h	d h	d h	d h	d h
	8 11·0	7 22·9	4 3·2	8 1·1	4 5·4	8 1·9	3 4·7
	17 4·0	16 16·2	12 20·7	16 18·6	12 22·7	16 18·9	11 21·4
27 18·2	25 21·1	25 9·7	21 14·1	25 12·1	21 15·8	25 11·8	20 13·9
			30 7·6		30 8·9		29 6·4

Oberon.

April.	May.	June.	July.	August.	September.
d h	d h	d h	d h	d h	d h
7 8·3	4 8·9	13 22·2	10 22·5	6 21·5	2 19·6
20 20·5	17 21·4	27 10·5	24 10·1	20 8·7	16 6·1
	31 9·8				

Apparent Apsides.

		Apparent Distances.			
	Position Angle.	Ariel.	Umbriel.	Titania.	Oberon.
	°	″	″	″	″
March 8…………	325·4	14·1	19·6	32·2	43·1
May 27 …………	332·3	14·7	20·5	33·7	45·0
August 15 ………	337·9	14·1	19·7	32·2	43·1

SATELLITE OF NEPTUNE.

Times of passage through the east apse of apparent orbit.

January.	February.	March.	September.	October.	November.	December.
d h	d h	d h	d h	d h	d h	d h
0 16·1	4 22·7	6 8·1	4 10·2	3 19·3	2 4·7	1 14·1
6 13·2	10 19·8	12 5·2	10 7·2	9 16·4	8 1·8	7 11·3
12 10·3	16 16·9		16 4·3	15 13·4	13 22·9	13 8·4
18 7·4	22 14·0		22 1·3	21 10·5	19 19·9	19 5·5
24 4·5	28 11·1		27 22·3	27 7·6	25 17·0	25 2·6
30 1·6						30 23·7

Position-angle and distance of the apse:—Feb. 4, 257°·8, 16″·7; Sept. 28, *263°·2, 16″·9; December 17,* 261°·6, 16″·9.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF THE SUN.

Greenwich Mean Noon.				Greenwich Mean Noon.			
1899.	P.	D.	L.	1899.	P.	D.	L.
	° ′	° ′	° ′		° ′	° ′	° ′
Jan. 1	+ 1 46	−3 17	171 36	July 5	− 0 41	+3 32	249 45
6	− 0 40	3 50	105 45	10	+ 1 37	4 3	183 35
11	3 5	4 22	39 54	15	3 51	4 32	117 25
16	5 28	4 52	334 4	20	6 4	4 59	51 15
21	7 44	5 19	268 14	25	8 13	5 24	345 7
26	9 59	5 44	202 24	30	10 17	5 47	278 59
31	12 6	6 6	136 34	Aug. 4	12 16	6 8	212 52
Feb. 5	14 7	6 26	70 45	9	14 10	6 26	146 45
10	16 0	6 42	4 55	14	15 57	6 42	80 39
15	17 45	6 55	299 5	19	17 37	6 54	14 34
20	19 21	7 5	233 14	24	19 10	7 4	308 30
25	20 48	7 11	167 23	29	20 35	7 11	242 26
Mar. 2	22 6	7 15	101 31	Sept. 3	21 52	7 14	176 23
7	23 14	7 15	35 38	8	23 0	7 15	110 21
12	24 13	7 11	329 44	13	23 59	7 12	44 20
17	25 2	7 4	263 50	18	24 49	7 6	338 20
22	25 40	6 54	197 55	23	25 30	6 58	272 20
27	26 7	6 41	131 58	28	26 0	6 46	206 21
Apr. 1	26 24	6 25	66 0	Oct. 3	26 20	6 31	140 22
6	26 30	6 7	0 2	8	26 29	6 13	74 25
11	26 25	5 45	294 2	13	26 28	5 52	8 28
16	26 9	5 21	228 1	18	26 15	5 28	302 31
21	25 42	4 55	161 58	23	25 50	5 2	236 34
26	25 4	4 27	95 54	28	25 14	4 34	170 38
May 1	24 15	3 57	29 49	Nov. 2	24 26	4 3	104 42
6	23 15	3 26	323 43	7	23 26	3 31	38 47
11	22 4	2 52	257 37	12	22 14	2 57	332 52
16	20 44	2 18	191 29	17	20 51	2 22	266 57
21	19 13	1 44	125 20	22	19 17	1 45	201 2
26	17 34	1 8	59 11	27	17 32	1 7	135 9
31	15 45	−0 32	353 1	Dec. 2	15 37	+0 29	69 16
June 5	13 50	+0 4	286 51	7	13 34	−0 9	3 23
10	11 48	0 40	220 40	12	11 23	0 47	297 30
15	9 41	1 16	154 29	17	9 6	1 26	231 38
20	7 28	1 52	88 18	22	6 45	2 3	165 46
25	5 14	2 26	22 7	27	4 20	2 40	99 55
30	− 2 58	+3 0	315 56	32	+ 1 54	−3 15	34 4

The position-angle of the Sun's axis, P, is the position-angle of the N. end of the axis from the N. point of the Sun, read in the direction N., E., S., W. In computing D (the heliographic latitude of the centre of the Sun's disk), the inclination of the Sun's axis to the ecliptic has been assumed to be 82° 45′, and the longitude of the ascending node to be 74° 23′. In computing L (the heliographic longitude of the centre of the disk), the Sun's period of rotation has been assumed to be 25·38 days, and the meridian which passed through the ascending node at the epoch 1854·0 has been taken as the zero meridian.

MEAN PLACES OF VARIABLE STARS FOR 1899·0.

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
1.	V Sculptoris ...	0 3 31	−39 47·5	54.	R Columbæ......	5 46 38	−29 13·2
2.	S Sculptoris ...	0 10 16	−32 36·4	55.	U Orionis	5 49 49	+20 9·4
3.	T Andromedæ .	0 17 7	+26 26·1	56.	η Geminorum...	6 8 47	+22 32·2
4.	T Cassiopeiæ ...	0 17 46	+55 14·0	57.	V Aurigæ	6 16 14	+47 42·7
5.	S Toucani	0 18 21	−62 14·3	58.	V Monocerotis..	6 17 38	− 2 8·7
6.	R Andromedæ .	0 18 42	+38 1·1	59.	T Monocerotis...	6 19 46	+ 7 8·5
7.	S Ceti	0 18 55	− 9 53·3	60.	W Geminorum .	6 29 10	+15 24·5
8.	T Sculptoris ...	0 24 14	−38 28·0	61.	S Monocerotis .	6 35 25	+ 9 59·1
9.	U Cassiopeiæ ...	0 40 42	+47 42·3	62.	W Monocerotis .	6 47 27	− 7 1·5
10.	V Andromedæ .	0 44 36	+35 6·2	63.	R Lyncis.........	6 52 58	+55 28·4
11.	X Sculptoris ...	0 44 39	−35 28·1	64.	ζ Geminorum ...	6 58 7	+20 43·1
12.	W Cassiopeiæ...	0 48 55	+58 1·0	65.	R Geminorum .	7 1 17	+22 51·6
13.	U Cephei	0 53 19	+81 19·9	66.	V Canis Min. ...	7 1 30	+ 9 1·6
14.	U Sculptoris ...	1 6 47	−30 39·1	67.	R Canis Min. ...	7 3 9	+10 11·0
15.	U Andromedæ .	1 9 44	+40 11·1	68.	L² Puppis	7 10 27	−44 28·7
16.	S Cassiopeiæ ...	1 12 13	+72 4·8	69.	R Canis Maj. ...	7 14 54	−16 12·3
17.	S Piscium	1 12 17	+ 8 23·9	70.	V Geminorum...	7 17 30	+13 17·2
18.	U Piscium	1 17 38	+12 20·3	71.	U Monocerotis..	7 25 59	− 9 33·9
19.	R Sculptoris ...	1 22 19	−33 4·0	72.	S Canis Min. ...	7 27 14	+ 8 32·1
20.	R Piscium	1 25 25	+ 2 21·6	73.	T Canis Min. ...	7 28 23	+11 57·6
21.	X Cassiopeiæ ...	1 49 41	+58 45·8	74.	X Puppis	7 28 24	−20 41·6
22.	U Persei	1 52 52	+54 19·9	75.	U Canis Min. ...	7 35 51	+ 8 37·0
23.	S Arietis.........	1 59 13	+12 2·5	76.	S Geminorum ..	7 36 59	+23 41·3
24.	R Arietis.........	2 10 22	+24 35·2	77.	T Geminorum ..	7 43 15	+23 59·1
25.	o Ceti	2 14 15	− 3 26·1	78.	U Geminorum .	7 49 7	+22 16·0
26.	R Ceti............	2 20 52	− 0 38·1	79.	V Puppis	7 55 20	−48 58·3
27.	S Horologii......	2 22 21	−60 1·5	80.	U Puppis	7 56 5	−12 33·7
28.	U Ceti............	2 28 53	−13 35·5	81.	R Cancri.........	8 11 0	+12 2·2
29.	R Trianguli ...	2 30 55	+33 49·5	82.	V Cancri.........	8 15 58	+17 36·3
30.	T Arietis.........	2 42 41	+17 5·3	83.	V Carinæ	8 26 40	−59 46·9
31.	V Persei	2 43 10	+56 33·8	84.	X Carinæ	8 29 5	−58 53·0
32.	R Horologii ...	2 49 40	−50 21·5	85.	U Cancri.........	8 29 59	+19 14·7
33.	Algol	3 1 35	+40 34·0	86.	T Velorum	8 34 24	−47 0·5
34.	U Arietis	3 5 27	+14 25·0	87.	S Cancri.........	8 38 10	+19 23·9
35.	R Persei	3 23 37	+35 19·4	88.	R Mali	8 41 15	−27 50·0
36.	T Eridani	3 50 54	−24 19·7	89.	S Hydræ.........	8 48 18	+ 3 27·0
37.	λ Tauri	3 55 5	+12 12·4	90.	T Hydræ.........	8 50 45	− 8 45·4
38.	W Tauri.........	4 22 15	+15 52·9	91.	T Cancri.........	8 50 54	+20 14·2
39.	R Tauri	4 22 46	+ 9 56·2	92.	S Mali............	9 0 38	−24 41·1
40.	S Tauri	4 23 40	+ 9 43·3	93.	W Cancri	9 3 59	+25 39·5
41.	T Camelopardi .	4 30 16	+65 56·6	94.	W Carinæ	9 19 13	−55 31·7
42.	R Reticuli	4 32 30	−63 14·3	95.	S Antliæ	9 27 53	−28 11·0
43.	V Tauri	4 46 11	+17 22·1	96.	N Velorum......	9 28 9	−56 45·3
44.	R Orionis	4 53 32	+ 7 58·7	97.	U Velorum......	9 29 25	−45 4·0
45.	R Leporis	4 55 0	−14 57·5	98.	S Velorum	9 29 25	−44 45·7
46.	T Leporis	5 0 32	−22 2·5	99.	R Carinæ	9 29 42	−62 20·5
47.	V Orionis	5 0 44	+ 3 57·9	100.	X Hydræ	9 30 41	−14 14·4
48.	S Pictoris	5 8 16	−48 37·8	101.	R Leo. Min. ...	9 39 32	+34 58·7
49.	R Aurigæ	5 9 8	+53 28·4	102.	R Leonis	9 42 8	+11 53·9
50.	T Columbæ......	5 15 35	−33 48·8	103.	*l* Carinæ.........	9 42 28	−62 2·5
51.	S Orionis.........	5 24 1	− 4 46·4	104.	V Leonis	9 54 25	+21 44·7
52.	S Camelopardi .	5 30 7	+68 44·6	105.	S Carinæ.........	10 6 9	−61 3·3
53.	U Aurigæ	5 35 35	+31 59·4	106.	Y Carinæ	10 29 23	−57 58 6

Mean Places of Variable Stars for 1899·0 (continued).

No.	Star.	R.A.	Dec.
		h m s	° ′
107.	R Ursæ Maj.	10 37 30	+69 18·3
108.	V Hydræ	10 46 42	−20 42·1
109.	W Leonis	10 48 18	+14 15·2
110.	U Carinæ	10 53 41	−59 11·5
111.	S Leonis	11 5 38	+ 6 0·5
112.	S² Centauri	11 16 3	−61 19·3
113.	X Centauri	11 44 9	−41 11·6
114.	W Centauri	11 49 59	−58 41·5
115.	R Comæ	11 59 4	+19 20·9
116.	S Muscæ	12 7 21	−69 35·4
117.	T Virginis	12 9 26	− 5 28·5
118.	R Corvi	12 14 24	−18 41·6
119.	T Crucis	12 15 50	−61 43·3
120.	R Crucis	12 18 5	−61 4·1
121.	Y Virginis	12 28 40	− 3 51·9
122.	T Ursæ Maj.	12 31 48	+60 2·6
123.	R Virginis	12 33 23	+ 7 32·6
124.	R Muscæ	12 35 55	−68 51·2
125.	S Ursæ Maj.	12 39 32	+61 38·8
126.	U² Virginis	12 42 10	+ 4 41·8
127.	U Virginis	12 45 58	+ 6 6·2
128.	S Crucis	12 48 23	−57 52·9
129.	W Virginis	13 20 49	− 2 51·3
130.	V Virginis	13 22 35	− 2 39·0
131.	R Hydræ	13 24 12	−22 45·5
132.	S Virginis	13 27 44	− 6 40·5
133.	Z Centauri	13 34 13	−31 7·3
134.	T Centauri	13 35 58	−33 5·2
135.	W Hydræ	13 43 20	−27 51·7
136.	R Canum Ven.	13 44 37	+40 2·7
137.	R² Virginis	13 59 32	− 8 42·8
138.	Z Virginis	14 4 54	−12 49·5
139.	R Centauri	14 9 18	−59 26·6
140.	R² Centauri	14 9 51	−57 23·0
141.	Y Boötis	14 17 19	+20 16·0
142.	X Boötis	14 19 24	+16 46·7
143.	S Boötis	14 19 30	+54 16·2
144.	S² Virginis	14 22 13	+ 5 7·9
145.	R Camelopardi	14 25 11	+84 17·4
146.	V Centauri	14 25 19	−56 26·4
147.	V Boötis	14 25 41	+39 18·7
148.	R Boötis	14 32 44	+27 10·5
149.	V Libræ	14 34 44	−17 13·3
150.	S Lupi	14 46 38	−46 11·7
151.	U Boötis	14 49 39	+18 6·4
152.	δ Libræ	14 55 35	− 8 7·1
153.	T Triang. Austr.	15 0 19	−68 19·9
154.	T Libræ	15 4 58	−19 38·0
155.	Y Libræ	15 6 21	− 5 37·8
156.	R Triang. Austr.	15 10 44	−66 7·5
157.	U Coronæ	15 14 5	+32 1·0
158.	S Libræ	15 15 35	−20 1·4
159.	S Serpentis	15 16 56	+14 40·6
160.	S Coronæ	15 17 17	+31 43·8
161.	S² Libræ	15 18 26	−22 33·0
162.	U² Libræ	15 27 38	−14 59·1
163.	X Libræ	15 30 22	−20 49·7
164.	W Libræ	15 32 9	−15 50·4
165.	S Ursæ Majoris	15 33 29	+78 58·5
166.	U Libræ	15 36 10	−20 51·3
167.	Z Libræ	15 40 38	−20 48·4
168.	V Coronæ	15 45 55	+39 52·5
169.	R Serpentis	15 46 2	+15 26·4
170.	R Libræ	15 47 53	−15 56·1
171.	R² Libræ	15 50 35	−18 0·4
172.	S Triang. Austr.	15 52 7	−63 29·3
173.	U Triang. Austr.	15 58 20	−62 38·1
174.	X Herculis	15 59 37	+47 30·9
175.	Z Scorpii	16 0 4	−21 27·5
176.	R Herculis	16 1 41	+18 38·6
177.	X Scorpii	16 2 36	−21 15·3
178.	W Scorpii	16 5 52	−19 52·4
179.	S Normæ	16 10 30	−57 39·0
180.	R Scorpii	16 11 38	−22 41·7
181.	S Scorpii	16 11 39	−22 38·6
182.	W Ophiuchi	16 15 58	− 7 27·5
183.	V Ophiuchi	16 21 6	−12 11·9
184.	U Herculis	16 21 21	+19 7·1
185.	Y Scorpii	16 23 46	−19 13·1
186.	T Ophiuchi	16 27 57	−15 55·2
187.	S Ophiuchi	16 28 27	−16 56·9
188.	R Aræ	16 31 21	−56 47·5
189.	W Herculis	16 31 39	+37 32·8
190.	Y Herculis	16 31 57	+ 7 18·7
191.	R Draconis	16 32 23	+66 57·9
192.	S Herculis	16 47 18	+15 6·7
193.	S² Scorpii	16 48 18	−44 56·2
194.	R² Scorpii	16 50 11	−30 25·1
195.	V² Scorpii	16 51 43	−33 27·1
196.	R Ophiuchi	17 1 58	−15 57·5
197.	W² Scorpii	17 8 14	−33 18·9
198.	U Ophiuchi	17 11 24	+ 1 19·3
199.	Z Ophiuchi	17 14 25	+ 1 37·4
200.	S² Herculis	17 17 28	+23 1·2
201.	U² Scorpii	17 35 2	−43 42·3
202.	X Sagittarii	17 41 12	−27 47·5
203.	Y² Scorpii	17 44 12	−33 40·5
204.	Y Ophiuchi	17 47 14	− 6 7·1
205.	Z Herculis	17 53 33	+15 8·4
206.	T Draconis	17 54 51	+58 13·7
207.	W Sagittarii	17 58 35	−29 35·2
208.	T Herculis	18 5 17	+31 0·1
209.	S² Sagittarii	18 10 54	−34 8·5
210.	W Lyræ	18 11 26	+36 38·3
211.	Y Sagittarii	18 15 27	−18 54·3
212.	V² Sagittarii	18 21 17	−33 22·7
213.	*d* Serpentis	18 22 2	+ 0 8·2
214.	T Serpentis	18 23 53	+ 6 13·8
215.	U Sagittarii	18 25 56	−19 11·9
216.	X Ophiuchi	18 33 32	+ 8 44·4

Mean Places of Variable Stars for 1899·0 (continued).

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
217.	U Coron. Austr.	18 34 13	−37 55·7	262.	X Cygni	20 39 27	+35 13·3
218.	R Scuti	18 42 6	− 5 48·9	263.	T Delphini	20 40 40	+16 1·9
219.	β Lyræ	18 46 21	+33 14·7	264.	W Aquarii	20 41 7	− 4 27·1
220.	κ Pavonis	18 46 32	−67 21·6	265.	V Aquarii	20 41 43	+ 2 4·1
221.	R Lyræ	18 52 16	+43 48·7	266.	U Capricorni	20 42 31	−15 9·3
222.	R Coron. Austr.	18 55 5	−37 5·8	267.	R² Cygni	20 42 35	+44 30·0
223.	R Aquilæ	19 1 30	+ 8 4·7	268.	V Delphini	20 43 11	+18 57·8
225.	V Lyræ	19 5 7	+29 29·9	269.	T Aquarii	20 44 37	− 5 31·3
226.	S Lyræ	19 9 4	+25 49·9	270.	U Pavonis	20 47 5	−63 5·4
227.	W Aquilæ	19 9 57	− 7 13·1	271.	T Vulpeculæ	20 47 11	+27 52·1
228.	T Sagittarii	19 10 25	−17 8·9	272.	Y Cygni	20 48 2	+34 16·7
229.	R Sagittarii	19 10 46	−19 29·1	273.	X Delphini	20 50 15	+17 14·1
230.	S Sagittarii	19 13 31	−19 12·5	274.	R Vulpeculæ	20 59 54	+23 25·3
231.	Z Sagittarii	19 13 44	−21 6·7	275.	V Capricorni	21 1 44	−24 19·9
232.	T Sagittæ	19 17 12	+17 27·7	276.	X Capricorni	21 2 46	−21 45·3
233.	U Aquilæ	19 23 55	− 7 15·1	277.	Z Capricorni	21 5 0	−16 35·4
234.	U Vulpeculæ	19 32 13	+20 6·5	278.	T Cephei	21 8 13	+68 4·8
235.	T² Aquilæ	19 33 14	+11 28·8	279.	T Capricorni	21 16 27	−15 35·1
236.	R Cygni	19 34 7	+49 58·4	280.	Y Capricorni	21 28 52	−14 25·3
237.	T Pavonis	19 39 24	−72 0·8	281.	W Cygni	21 32 14	+44 55·4
238.	T³ Cygni	19 40 46	+29 0·2	282.	S Cephei	21 36 29	+78 10·2
239.	T² Cygni	19 40 48	+48 31·7	283.	U² Cygni	21 37 14	+53 52·0
240.	S Vulpeculæ	19 44 15	+27 2·1	284.	R³ Cygni	21 38 44	+43 7·3
241.	X Aquilæ	19 46 28	+ 4 12·4	285.	R Gruis	21 42 2	−47 22·3
242.	χ Cygni	19 46 41	+32 39·5	286.	U Aquarii	21 57 49	−17 6·8
243.	η Aquilæ	19 47 20	+ 0 44·8	287.	S Piscis Austr.	21 57 59	−28 32·3
244.	R² Sagittarii	19 49 39	−29 27·2	288.	T Pegasi	22 3 58	+12 2·8
245.	S Sagittæ	19 51 25	+16 22·0	289.	R Piscis Austr.	22 12 16	−30 6·5
246.	R² Aquilæ	19 52 22	− 2 9·3	290.	X Aquarii	22 13 6	−21 24·3
247.	Z Cygni	19 58 36	+49 45·7	291.	S Gruis	22 19 51	−48 57·1
248.	S Cygni	20 3 23	+57 41·2	292.	S Lacertæ	22 24 35	+39 47·9
249.	R Capricorni	20 5 39	−14 34·2	293.	δ Cephei	22 25 25	+57 53·9
250.	S Aquilæ	20 6 59	+15 19·2	294.	W Cephei	22 32 37	+57 54·3
251.	W Capricorni	20 8 32	−22 17·1	295.	R Lacertæ	22 38 48	+41 50·6
252.	R Sagittæ	20 9 27	+16 25·2	296.	S Aquarii	22 51 42	−20 53·0
253.	Z Aquilæ	20 9 48	− 6 27·6	297.	R Pegasi	23 1 35	+ 9 59·9
254.	R Delphini	20 10 3	+ 8 47·0	298.	V Cassiopeiæ	23 7 20	+59 8·1
255.	T² Sagittarii	20 11 1	−39 25·4	299.	S Pegasi	23 15 26	+ 8 22·0
256.	U Cygni	20 16 28	+47 34·5	300.	R Aquarii	23 38 36	−15 50·6
257.	W Delphini	20 33 4	+17 55·7	301.	V Cephei	23 51 41	+82 37·7
258.	R Microscopii	20 33 54	−29 8·8	302.	V Ceti	23 52 44	− 9 31·4
259.	V Cygni	20 38 3	+47 46·8	303.	U Pegasi	23 52 49	+15 23·6
260.	S Delphini	20 38 26	+16 43·5	304.	R Cassiopeiæ	23 53 16	+50 49·6
261.	Y Aquarii	20 39 6	− 5 12·2	305.	W Ceti	23 56 57	−15 14·3

The variability of Y Herculis is doubtful. Y Aquilæ has been taken from list in accordance with recent investigations (see Ast. Nach. 3523).

RIABLES OF SHORT PERIOD NOT OF THE ALGOL TYPE.

(The No. after Star's Name refers to Catalogue on pp. 24–26.)

e times of minima only are given; the times of maxima may be found by adding the interval printed under the name of the star.

ER. 59.

h.)

	Sept.	
1	d	h
1	22	2
	Oct.	
1	19	3
·	Nov.	
1	15	3
2		
.	Dec.	
2	12	3

OR. 60.

h.)

.	Oct.	
h	d	h
6	8	13
0	16	7
	24	0
y	31	18
h		
8		
1	Nov.	
	8	12
;	16	6
	24	0
5		
9		
2	Dec.	
10		
	1	1
	9	17
t.	17	1
14	24	25
8		3
1		
19		

OR. 64.

h.)

a	Apr.	
7	d	h
	3	10
r.	13	14
h	23	18
10		
14		
18		

ζ GEMINOR. 64 (*continued*).

May		Sept.		Nov.	
d	h	d	h	d	h
3	8	2	5	2	3
13	12	12	8	12	7
23	16	22	12	22	10
Aug.		Oct.		Dec.	
12	21	2	16	2	14
23	1	12	20	12	18
		22	23	22	21

W VIRGINIS 129.

(8d 5h.)

Jan.		May		Sept.	
d	h	d	h	d	h
13	23	14	20	12	18
31	5	June		30	0
Feb.		1	3	Oct.	
17	12	18	9	17	7
Mar.		July		Nov.	
6	18	5	16	3	13
24	1	22	22	20	20
Apr.		Aug.		Dec.	
10	7	9	5	8	2
27	14	26	11	25	9

X SAGITTAR. 202.

(2d 21h.)

Feb.		May		Sept.	
d	h	d	h	d	h
1	12	17	16	6	20
8	12	24	16	13	21
15	12	31	16	20	21
22	12	June		27	21
Mar.		7	17	Oct.	
1	13	14	17	4	22
8	13	21	17	11	22
15	13	28	18	18	22
22	14	July		25	22
29	14	5	18	Nov.	
Apr.		12	18	1	23
5	14	19	18	8	23
12	14	26	19	15	23
19	15	Aug.		23	0
26	15	2	19	30	0
May		9	19	Dec.	
3	15	16	20	7	0
10	16	23	20		
		30	20		

Y OPHIUCHI 204.

(6d 5h.)

Jan.		Apr.		Aug.	
d	h	d	h	d	h
8	4	3	18	1	15
25	7	20	21	18	18
Feb.		May		Sept.	
11	10	8	0	4	20
28	13	25	3	21	23
Mar.		June		Oct.	
17	15	11	6	9	2
		28	9	26	5
		July			
		15	12		

W SAGITTAR. 207.

(3d 0h.)

Jan.		May		Sept.	
d	h	d	h	d	h
31	11	2	14	1	2
Feb.		10	4	8	16
8	1	17	18	16	7
15	15	25	9	23	21
23	5	June		Oct.	
Mar.		1	23	1	11
2	20	9	13	9	1
10	10	17	3	16	16
18	0	24	18	24	6
25	14	July		31	20
Apr.		2	8	Nov.	
2	5	9	22	8	11
9	19	17	12	16	1
17	9	25	3	23	15
24	23	Aug.			
		1	17		
		9	7		
		16	22		
		24	12		

Y SAGITTAR. 211.

(1d 19h.)

Jan.		May		Sept.	
d	h	d	h	d	h
21	8	3	3	3	11
27	2	8	21	9	6
Feb.		14	16	15	1
1	21	20	10	20	19
7	15	26	5	26	14
13	10	June		Oct.	
19	5	3	3	2	8
29	23	8	21	8	3
Mar.		14	16	13	21
2	18	20	10	19	16
8	12	26	5	25	10
14	7	July		31	5
20	1	1	23	Nov.	
25	20	7	18	6	0
31	14	13	12	11	18
Apr.		19	7	17	13
6	9	25	2	23	7
11	4	30	20	29	2
17	22	Aug.		Dec.	
23	17	5	15	4	20
29	11	11	9		
		17	4		
		22	22		
		28	17		

U SAGITTAR. 215.

(2d 23h.)

Jan.		Mar.		Apr.	
d	h	d	h	d	h
14	5	2	10	18	15
20	22	9	3	25	9
27	16	15	21	May	
Feb.		22	15	2	2
3	10	29	9	8	20
10	4	Apr.		15	14
16	22	5	3	22	8
23	16	11	21	29	2

U SAGITTAR. 215 (*continued*).

June		Aug.		Oct.	
d	h	d	h	d	h
4	20	4	13	4	5
11	14	11	6	10	23
18	8	18	0	17	17
25	1	24	18	24	11
July		31	12	31	5
1	19	Sept.		Nov.	
8	13	7	6	6	23
15	7	14	0	13	17
22	1	20	18	20	11
28	19	27	12	27	4

β LYRÆ 219.

(Max. 3d 2h.
Secondary Min.
6d 11h.
Max. 9d 16h.)

Jan.		May		Sept.	
d	h	d	h	d	h
11	9	7	15	13	19
24	7	20	13	26	17
Feb.		June		Oct.	
6	5	2	11	9	15
19	3	15	9	22	13
Mar.		28	7	Nov.	
4	1	July		4	11
16	23	11	5	17	9
29	21	24	3	30	7
Apr.		Aug.		Dec.	
11	19	6	11	13	5
24	17	18	23	26	3
		31	21		

VARIABLES OF SHORT PERIOD (*continued*).

U AQUILÆ 233.

($2^{d}\ 4^{h}$.)

Feb.		May		Sept.	
d	h	d	h	d	h
12	11	21	19	4	4
19	12	28	20	11	4
26	12	June		18	5
Mar.		4	20	25	5
5	13	11	21	Oct.	
12	13	18	21	2	6
19	14	25	22	9	7
26	14	July		16	7
Apr.		2	22	23	8
2	15	9	23	30	8
9	16	17	0	Nov.	
16	16	24	0	6	9
23	17	31	1	13	9
30	17	Aug.		20	10
May		7	1	27	11
7	18	14	2		
14	18	21	2		
		28	3		

η AQUILÆ 243.

($2^{d}\ 6^{h}$.)

Feb.		Mar.		Apr.	
d	h	d	h	d	h
2	15	3	8	1	1
9	19	10	12	8	5
16	23	17	16	15	9
24	4	24	21	22	13

η AQUILÆ 243 (*continued*).

Apr.		July		Oct.	
d	h	d	h	d	h
29	18	3	8	4	15
May		10	12	11	19
6	22	17	16	18	23
14	2	24	21	26	4
21	6	Aug.		Nov.	
28	11	1	1	2	8
June		8	5	9	12
4	15	15	9	16	16
11	19	22	13	23	20
18	23	29	18	Dec.	
26	4	Sept.		1	1
		5	22	8	5
		13	2	15	9
		20	6	22	13
		27	11	29	18

S SAGITTÆ 245.

($3^{d}\ 10^{h}$.)

Jan.		Mar.		Apr.	
d	h	d	h	d	h
25	15	8	13	19	11
Feb.		16	22	27	20
3	0	25	7	May	
11	9	Apr.		6	5
19	18	2	16	14	14
28	3	11	1	22	23
				31	9

S SAGITTÆ 245 (*continued*).

June		Aug.		Oct.	
d	h	d	h	d	h
8	18	6	10	4	3
17	3	14	19	12	12
25	12	23	5	20	21
July		31	14	29	6
3	21	Sept.		Nov.	
12	7	8	23	6	15
20	16	17	8	15	1
29	1	25	18	23	10
				Dec.	
				1	19
				10	4

X CYGNI 262.

($6^{d}\ 19^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
2	8	13	10	5	3
17	18	29	20	21	12
Feb.		June		Oct.	
4	3	14	5	7	22
20	12	July		24	7
Mar.		1	14	Nov.	
8	21	17	23	9	16
25	7	Aug.		26	1
Apr.		3	9	Dec.	
10	16	19	18	12	11
27	1			28	20

T VULPEC. 271.

($1^{d}\ 9\frac{1}{2}^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
1	4	5	9	2	4
5	15	9	20	6	14
10	1	14	6	11	1
14	12	18	17	15	11
18	22	23	3	19	22
23	9	27	14	24	8
27	19	June		28	19
Feb.		1	0	Oct.	
1	6	5	11	3	5
5	16	9	21	7	16
10	3	18	18	12	2
14	13	23	4	16	12
18	23	27	15	20	23
23	10	July		25	9
27	20	2	1	29	20
Mar.		6	12	Nov.	
4	7	10	23	3	6
8	17	15	9	7	17
13	4	19	19	12	3
17	14	24	6	16	14
22	1	28	16	21	0
26	11	Aug.		25	11
30	22	2	3	29	21
Apr.		6	13	Dec.	
4	8	11	0	4	8
8	19	15	10	8	18
13	5	19	20	13	5
17	15	24	7	17	15
22	2	28	17	22	1
26	12			26	12
30	23			30	22

δ CEPH

(1^{d} 14

Jan.		Ma
d	h	d
1	20	5
7	5	10
12	14	16
17	23	21
23	8	26
28	16	Jun
Feb.		1
3	1	6
8	10	11
13	19	17
19	3	22
24	12	27
Mar.		Jul
1	21	3
7	6	8
12	15	11
17	23	19
23	8	24
28	17	30
Apr.		Au
3	2	4
8	11	9
13	19	15
19	4	20
24	13	25
29	22	31

MAGNETIC ELEMENTS, ROYAL OBSERVATORY, GREENWICH.

Year.		Declination.	Horizontal force.	Dip.
		° ′		° ′
1870, observed	....	19 53 west.	0·1782	67 52
1880, ”	....	18 33 ”	0·1804	67 36
1890, ”	....	17 29 ”	0·1823	67 23
1899, inferred	....	16 33 ”	0·1839	67 10

The horizontal force is given in C. G. S. measure.

MINIMA OF VARIABLE STARS OF THE ALGOL TYPE.

(The figures following star's name are the Nos. in Catalogue, pp. 24–26.)

U CEPHEI 13 *.

($2^d\ 11^h\ 49\frac{1}{2}^m$.)

Month	d	h	m
Jan.	3	10	34
	8	10	14
	13	9	53
	18	9	32
	23	9	11
	28	8	50
Feb.	2	8	30
	7	8	9
	12	7	48
	17	7	27
	22	7	6
	27	6	46
March.	4	6	25
	9	6	4
	14	5	43
	19	5	22
	24	5	2
	29	4	41
April.	3	4	20
	8	3	59
	13	3	38
	18	3	17
	23	2	57
	28	2	36
May.	3	3	15
	8	1	54
	13	1	33
	18	1	13
	23	0	52
	28	0	31
June.	2	0	10
	6	23	49
	11	23	29
	16	23	8
	21	22	47
	26	22	26
July.	1	22	5
	6	21	44
	11	21	24
	16	21	3
	21	20	4
	26	20	21
	31	20	0
August.	5	19	40
	10	19	19
	15	18	58
	20	18	37
	25	18	16
	30	17	55
Sept.	4	17	35
	9	17	14
	14	16	53
	19	16	32
	24	16	11
	29	15	50
Oct.	4	15	30
	9	15	9
	14	14	48
	19	14	27
	24	14	6
	29	13	45
Nov.	3	13	25
	8	13	4
	13	12	43
	18	12	22
	23	12	1
	28	11	40
Dec.	3	11	20
	8	10	59
	13	10	38
	18	10	17
	23	9	56
	28	9	35

ALGOL 33 *.

($2^d\ 20^h\ 49^m$.)

Month	d	h	m
Jan.	2	7	14
	8	0	52
	13	18	30
	19	12	7
	25	5	45
	30	23	23
Feb.	5	17	1
	11	10	39
	17	4	17
	22	21	54
	28	15	32
March.	6	9	10
	12	2	48
	17	20	26
	23	14	4
	29	7	41
April.	4	1	19
	9	18	57
	15	12	35
	21	6	14
	26	23	50
June.	26	5	8
July.	1	22	35
	7	16	13
	13	9	51
	19	3	29
	24	21	7
	30	14	44
August.	5	8	22
	11	2	0
	16	19	38
	22	13	16
	28	6	54
Sept.	3	0	31
	8	18	9
	14	11	47
	20	5	25
	25	23	3
Oct.	1	16	40
	7	10	18
	13	3	56
	18	21	34
	24	15	12
	30	8	50
Nov.	5	2	27
	10	20	5
	16	13	43
	22	7	21
	28	0	59
Dec.	3	18	37
	9	12	14
	15	5	52
	20	23	30
	26	17	8

λ TAURI 37 *.

($3^d\ 22^h\ 52^m$.)

Month	d	h	m
Jan.	1	7	58
	9	5	42
	17	3	27
	25	1	11
Feb.	1	22	56
	9	20	40
	17	18	24
	25	16	9
March.	5	13	53
	13	11	38
	21	9	22
	29	7	6
July.	2	3	59
	10	1	44
	17	23	28
	25	21	12

λ TAURI 37 *.

(*continued*).

Month	d	h	m
August.	2	18	57
	10	16	41
	18	14	26
	26	12	10
Sept.	3	9	54
	11	7	39
	19	5	23
	27	3	8
Oct.	5	0	52
	12	22	36
Oct.	20	20	21
	28	18	5
Nov.	5	15	49
	13	13	33
	21	11	17
	29	9	3
Dec.	7	6	43
	15	4	32
	23	2	16
	31	0	0

R CANIS MAJ. 69†.

$\left\{\begin{matrix} 1 = 1^d\ 3^h\ 16^m \\ 2 = 2\ \ 6\ \ 31 \end{matrix}\right\}$

Month	d	h	m
Jan.	1	1	39
	4	11	27
	7	21	14
	11	7	1
	14	16	49
	18	2	36
	21	12	23
	24	22	11
	28	7	58
	31	17	45
Feb.	4	3	32
	7	13	20
	10	23	7
	14	8	54
	17	18	42
	21	4	29
	24	14	16
	28	0	4
March.	3	9	51
	6	19	38
	10	5	25
	13	15	12
March.	17	1	0
	20	10	47
	23	20	35
	27	6	22
	30	16	9
April.	3	2	57
	6	11	44
	9	21	31
	13	7	18
	16	17	6
	20	2	53
	23	12	40
	26	22	28
	30	8	15
May.	3	18	2
	7	3	14
	10	13	37
	13	23	24
	17	9	11
	20	18	59
	24	4	46
	27	14	33

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* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

† Every third minimum only.

Minima of Variable Stars of the Algol Type (continued).

R Canis Maj. 69†
(*continued*).

Sept.

d	h	m
1	3	53
4	13	41
7	23	28
11	9	15
14	19	3
18	4	50
21	14	37
25	0	25
28	10	12

Oct.

d	h	m
1	19	59
5	5	46
8	15	34
12	1	21
15	11	8
18	20	56
22	6	43
25	16	30
29	2	18

Nov.

d	h	m
1	12	5
4	21	52
8	7	39
11	17	27
15	3	14
18	13	1
21	22	49
25	8	36
28	18	23

Dec.

d	h	m
2	4	11
5	13	58
8	23	45
12	9	32
15	19	20
19	5	7
22	14	54
26	0	42
29	10	29

S Cancri 87.

Jan.

d	h	m
3	10	28
12	22	5
22	9	43
31	21	21

Feb.

d	h	m
10	8	59
19	20	36

March.

d	h	m
1	8	14
10	19	52
20	7	30
29	19	7

April.

d	h	m
8	6	45
17	18	23
27	6	1

May.

d	h	m
6	17	38
16	5	16
25	16	54

June.

d	h	m
4	4	32
13	16	29
23	3	47

July.

d	h	m
2	15	25
12	3	3
21	14	40
31	2	18

Sept.

d	h	m
7	0	49
16	12	17
26	0	5

Oct.

d	h	m
5	11	42
14	23	20
24	10	58

Nov.

d	h	m
2	22	36
12	10	13
21	21	51

Dec.

d	h	m
1	9	29
10	21	7
20	8	44
29	20	22

δ Libræ 152 *.
($2^d\ 7^h\ 51^m$.)

Jan.

d	h	m
2	18	3
7	9	46
12	1	28
16	17	11
21	8	54
26	0	37
30	16	20

Feb.

d	h	m
4	8	2
8	23	45
13	15	28
18	7	11
22	22	53
27	14	36

March.

d	h	m
4	6	19
8	22	2
13	13	44
18	5	27
22	21	10
27	12	53

April.

d	h	m
1	4	35
5	20	18
10	12	1
15	3	44
19	19	26
24	11	9
29	2	52

May.

d	h	m
3	18	35
8	10	17
15	9	52
20	1	34
24	17	17
29	9	0

June.

d	h	m
3	0	43
7	16	25
12	8	18
16	23	51
21	15	34
26	7	16
30	22	59

July.

d	h	m
5	14	42
10	6	25
14	22	7
19	13	50
24	5	33
28	21	16

August.

d	h	m
2	12	59
7	4	41
11	20	24
16	12	7
21	3	50
25	19	32
30	11	15

Sept.

d	h	m
4	2	58
8	18	41
13	10	23
18	2	6
22	17	49
27	9	32

Dec.

d	h	m
1	13	30
6	5	13
10	20	56
15	12	39
20	4	21
24	20	4
29	11	47

U Coronæ 157 *.
($3^d\ 10^h\ 51^m$.)

Jan.

d	h	m
4	6	45
11	4	27
18	2	9
24	23	51
31	21	33

Feb.

d	h	m
7	19	15
14	16	57
21	14	39
28	12	21

U Coronæ 157 *
(*continued*).

March.

d	h	m
7	10	3
14	7	45
21	5	28
28	3	10

April.

d	h	m
4	0	52
10	22	34
17	20	16
24	17	58

May.

d	h	m
1	15	40
8	13	41
15	11	4
22	8	46
29	6	28

June.

d	h	m
5	4	10
12	1	52
18	23	34
25	21	16

July.

d	h	m
2	18	58
9	16	40
16	14	22
23	12	4
30	9	46

August.

d	h	m
6	7	28
13	5	10
20	3	52
26	23	34

Sept.

d	h	m
2	22	17
9	19	59
16	17	41
23	15	23
30	13	5

Oct.

d	h	m
7	10	47
14	8	29
21	6	11
28	3	53

Nov.

d	h	m
4	1	35
10	23	17
17	20	59
24	18	41

Dec.

d	h	m
1	16	23
8	14	5
15	11	47
22	9	29
29	7	11

U Ophiuchi 198‡.

$$\left\{\begin{array}{l} 1 = 0^d\ 20^h\ 8^m \\ 2 = 1\ \ 16\ \ 16 \\ 3 = 2\ \ 12\ \ 23 \end{array}\right\}$$

Jan.

d	h	m
1	2	43
4	11	14
7	19	45
11	4	16
14	12	47
17	21	18
21	5	48
24	14	19
27	22	50
31	7	21

Feb.

d	h	m
3	15	52
7	0	23
10	8	53
13	17	24
17	1	55
20	10	26
23	18	57
27	3	28

March.

d	h	m
2	11	58
5	20	29
9	5	0
12	13	31
15	22	2
19	6	33
22	15	4
25	23	34
29	8	5

---

* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

† Every third minimum only.

‡ *Every fourth* minimum only.

Minima of Variable Stars of the Algol Type (continued).

U OPHIUCHI 198 ‡ (*continued*).

Month	d	h	m
April.	1	16	36
	5	1	7
	8	9	38
	11	18	9
	15	2	39
	18	11	10
	21	19	41
	25	4	12
	28	12	43
May.	1	21	14
	5	5	44
	8	14	15
	11	22	46
	15	7	17
	18	15	48
	22	0	19
	25	8	49
	28	17	20
June.	1	1	51
	4	10	22
	7	18	53
	11	3	24
	14	11	54
	17	20	25
	21	4	56
	24	13	27
	27	21	58
July.	1	6	29
	4	14	59
	7	23	30
	11	8	1
	14	16	32
	18	1	3
	21	9	34
	24	18	4
	28	2	35
	31	11	6
August.	3	19	37
	7	4	8
	10	12	30
	13	21	10
	17	5	40
	20	14	11
	23	22	42
	27	7	15
	30	15	44
Sept.	3	0	15
	6	8	45
	9	17	16
	13	1	47
	16	10	18
	19	18	49
	23	3	20
	26	11	50
	29	20	21
Oct.	3	4	52
	6	14	43
	9	21	54
	13	6	25
	16	15	55
	20	0	26
	23	8	57
	26	17	28
	30	1	59

Z HERCULIS 205 ‡.

$$\left\{\begin{array}{l}1=2^{d}\ \ 1^{h}\ 15^{m}\\ 2=3\ \ 23\ \ 49\\ 3=6\ \ \ 1\ \ \ 5\end{array}\right\}$$

Month	d	h	m
Jan.	2	11	4
	10	10	43
	18	10	22
	26	10	1
Feb.	3	9	40
	11	9	19
	19	8	58
	27	8	37

Z HERCULIS 205 ‡ (*continued*).

Month	d	h	m
March.	7	8	16
	15	8	56
	23	7	35
	31	7	14
April.	8	6	53
	16	6	32
	24	6	11
May.	2	5	50
	10	5	29
	18	5	8
	26	4	47
June.	3	4	27
	11	4	6
	19	3	45
	27	3	24
July.	5	3	3
	13	2	42
	21	2	21
	29	2	0
August.	6	1	39
	14	1	19
	22	0	58
	30	0	37
Sept.	7	0	15
	14	23	55
	22	23	34
	30	23	13
Oct.	8	22	52
	16	22	31
	24	22	11
Nov.	1	21	50
	9	21	29
	17	21	8
	25	20	47
Dec.	3	20	26
	11	20	5
	19	19	44
	27	19	23

W DELPHINI 257 †.

$$\left\{\begin{array}{l}1=4^{d}\ \ 19^{h}\ 21^{m}\\ 2=9\ \ \ 14\ \ \ 42\end{array}\right\}$$

Month	d	h	m
Jan.	5	10	16
	19	20	20
Feb.	2	6	23
	17	16	27
March.	4	2	30
	18	12	34
April.	1	22	38
	16	8	41
	30	18	45
May.	15	4	48
	29	14	52
June.	13	0	56
	27	10	59
July.	11	21	3
	26	7	6
Aug.	9	17	10
	24	3	14

W DELPHINI 257 † (*continued*).

Month	d	h	m
Sept.	7	13	17
	21	23	21
Oct.	6	9	24
	20	19	28
Nov.	4	5	32
	18	15	35
Dec.	3	1	39
	17	11	42
	31	21	46

Y CYGNI 272 ‡.

$$\left\{\begin{array}{l}1=1^{d}\ 20^{h}\ 46^{m}\\ 2=2\ \ 23\ \ 59\\ 3=4\ \ 20\ \ 41\end{array}\right\}$$

Month	d	h	m
Jan.	1	9	0
	7	8	49
	13	8	38
	19	8	27
	25	8	17
	31	8	6
Feb.	6	7	55
	12	7	44
	18	7	33
	24	7	22
March.	2	7	12
	8	7	1
	14	6	50
	20	6	39
	26	6	28
April.	1	6	18
	7	6	7
	13	5	56
	19	6	45
	25	5	34
May.	1	5	24
	7	5	13
	13	5	2
	19	4	51
	25	4	40
	31	4	30
June.	6	4	19
	13	4	8
	18	3	57
	24	3	46
	30	3	36
July.	6	3	25
	12	3	14
	18	3	3
	24	2	52
	30	2	42
August.	5	2	31
	11	2	20
	17	2	9
	23	1	58
	29	1	48
Sept.	4	1	37
	10	1	26
	16	1	15
	22	1	4
	28	0	53
Oct.	4	0	43
	10	0	32
	16	0	21
	22	0	10
	28	0	0

† Every third minimum only.
‡ Every fourth minimum only.

Minima of Variable Stars of the Algol Type (continued).

Y Cygni 272 ‡ (*continued*).

Nov.

d	h	m
2	23	49
8	23	38
14	23	27
20	23	16
26	23	5

Dec.

d	h	m
2	22	55
8	22	44
14	22	33
20	22	22
26	22	11

S Antliæ 95 §.

Jan.

d	h	m
1	3	55
4	9	43
7	15	31
10	21	19
14	3	7
17	8	55
20	14	43
23	20	31
27	2	19
30	8	7

Feb.

d	h	m
2	13	55
5	19	43

Feb.

d	h	m
9	1	31
12	7	19
15	13	7
18	18	55
22	0	43
25	6	31
28	12	19

March.

d	h	m
3	18	7
6	23	55
10	5	43
13	11	31
16	17	19

S Antliæ 95 § (*continued*).

March.

d	h	m
19	23	7
23	4	55
26	10	43
29	16	31

April.

d	h	m
1	22	19
5	4	7
8	9	55
11	15	43
14	21	31
18	3	19
21	9	7
24	14	55
27	20	43

May.

d	h	m
1	2	31
4	8	19
7	14	7
10	20	55
14	1	43
17	7	31

May.

d	h	m
20	13	19
23	19	7
27	0	55
30	6	43

June.

d	h	m
2	12	31
5	18	19
9	0	7
12	5	55
15	11	43
18	17	31
21	23	19
25	5	7
28	10	55

July.

d	h	m
1	16	43
4	22	31
8	4	19
11	10	7
14	16	55
17	21	43

S Antliæ 95 § (*continued*).

July.

d	h	m
21	3	31
24	9	19
27	15	7

Sept.

d	h	m
4	12	43
7	18	31
11	0	19
14	6	7
17	11	55
20	17	43
23	23	31
27	5	19
30	11	7

Oct.

d	h	m
3	16	55
6	22	43
10	4	31
13	10	19
16	16	7
19	21	55
23	3	43

Oct.

d	h	m
26	9	31
29	15	19

Nov.

d	h	m
1	21	7
5	2	55
8	8	43
11	14	31
14	20	19
18	2	7
21	7	55
24	13	43
27	19	31

Dec.

d	h	m
1	1	16
4	7	7
7	12	55
10	18	43
14	0	31
17	6	19
20	12	7
23	17	55
26	23	43
30	5	31

‡ Every fourth minimum only.

§ Every tenth minimum only; the others may be found by adding multiples of the period $7^{h}\ 46^{m}{\cdot}8$.

MAXIMA OF U PEGASI 303.

Jan.

d	h	m
1	2	38
4	20	35
8	14	32
12	8	29
16	2	26
19	20	22
23	14	19
27	8	16
31	2	13

Feb.

d	h	m
3	20	10
7	14	7
11	8	4
15	2	51
18	19	58
22	13	54
26	7	51

April.

d	h	m
8	13	17
12	7	14
16	1	11
19	19	8
23	13	5
27	7	2

May.

d	h	m
1	1	58
4	19	55
8	13	52
12	7	49
16	1	46
19	19	43
23	12	40
27	6	37
31	0	34

June.

d	h	m
3	18	30
7	12	27
11	6	24
15	0	21
18	18	18
22	12	15
26	6	12
30	0	9

July.

d	h	m
3	18	6
7	12	2
11	5	59
14	23	56
18	17	55
22	11	50
26	5	47
29	23	44

August.

d	h	m
2	17	41
6	11	38
10	5	34
13	23	31
17	17	28
21	11	25
25	5	22
28	23	19

Sept.

d	h	m
1	17	16
5	11	13
9	5	10
12	23	6
16	17	3
20	11	0
24	4	57
27	22	54

Oct.

d	h	m
1	16	51
5	10	48
9	4	45
12	22	42
16	16	39
20	10	35
24	4	32
27	22	19
31	16	16

Nov.

d	h	m
4	10	23
8	4	20
11	22	17
15	16	14
19	10	11
23	4	7
26	22	4
30	16	1

Dec.

d	h	m
4	9	58
8	3	55
11	21	52
15	15	49
19	9	46
23	3	43
26	21	39
30	15	36

Every fifteenth maximum is given; the others may be found by adding *multiples* of the period $5^{h}\ 32^{m}\ 2^{s}$.

The minimum takes place $2^{h}\ 28^{m}$ after the maximum.

MAXIMA AND MINIMA OF VARIABLE STARS.

The No. refers to Catalogue on pp. 24–26; M signifies maximum; *m*, minimum.

February (*con.*).

d	No.	
18	47	M
	206	*m*
20	111	*m*
	244	M
	150	M
	3	*m*
21	71	*m*
23	163	M
	191	*m*
	135	*m*
	103	*m*
24	28	M
	149	M
	148	*m*
26	259	*m*
	304	M
27	221	*m*
	260	*m*
28	125	*m*

March.

d	No.	
1	101	*m*
2	99	*m*
4	26	M
	160	M
5	263	M
	239	M
7	18	*m*
	158	M
8	154	M
	269	M
	247	*m*
	177	M
	289	M
	134	M
9	103	M
	123	*m*
	180	*m*
10	40	M
11	71	M
12	284	M
	161	M
	292	M
	240	M
14	156	*m*
15	252	*m*
	250	M
16	75	M
	122	*m*
	174	*m*
20	280	M
21	264	M
	253	*m*
	130	M
	17	M
	221	M
22	282	*m*
	283	*m*
	105	M
23	85	M
25	16	*m*
28	93	M
	276	M

March (*con.*).

d	No.	
29	266	M
30	68	*m*
31	103	*m*
	252	M
	183	M

April.

d	No.	
1	43	M
2	251	M
5	4	M
6	216	M
	230	*m*
	254	M
7	55	M
	298	*m*
	21	*m*
9	71	*m*
	6	*m*
	164	M
11	169	*m*
	90	M
13	103	M
14	63	*m*
	221	*m*
	151	*m*
15	258	M
	19	*m*
16	279	M
18	80	M
20	229	*m*
	76	*m*
	275	M
21	281	M
22	240	*m*
	299	*m*
24	107	*m*
25	50	M
26	7	M
	145	M
	134	*m*
27	71	M
	39	*m*
28	237	M
29	136	M
30	274	*m*
	208	M

May.

d	No.	
3	113	*m*
	48	M
	241	M
4	167	M
	89	M
5	181	M
	143	M
	103	*m*
6	221	M
8	297	*m*
9	127	M
10	134	*m*
11	307	*m*
	24	M

May (*con.*).

d	No.	
12	42	M
	8	M
	73	M
13	121	M
	12	*m*
14	25	*m*
15	256	M
	242	M
	174	M
17	123	M
	44	*m*
	163	*m*
19	103	M
	300	*m*
	240	M
	223	*m*
20	35	*m*
	147	M
	11	M
23	82	*m*
	70	*m*
24	105	*m*
	252	*m*
25	71	*m*
27	23	M
	57	M
28	196	*m*
29	18	M
	250	*m*
	192	*m*
30	221	*m*

June.

d	No.	
1	77	M
	232	*m*
	253	M
2	45	*m*
	49	M
4	226	M
	56	*m*
	117	M
5	114	M
	65	M
6	148	M
	170	M
7	158	*m*
	20	*m*
	52	M
	68	M
	134	M
9	26	*m*
	252	M
10	103	*m*
11	62	M
12	71	M
	67	*m*
	159	M
14	2	*m*
15	191	M
	3	M
	206	M
	155	M
16	239	*m*

June (*con.*).

d	No.	
17	174	*m*
	189	M
	53	*m*
	28	*m*
18	270	M
19	30	*m*
20	120	M
	112	M
21	125	M
	281	*m*
22	221	M
23	58	M
	103	M
25	111	M
	260	M
26	132	M
	102	M
27	194	*m*
	288	M
	51	M
	104	M
28	43	*m*
	278	*m*
29	258	*m*
	240	*m*
	275	M
30	100	M
	180	M

July.

d	No.	
1	274	M
	280	*m*
2	122	M
	277	*m*
	269	*m*
5	276	*m*
6	273	M
	40	*m*
9	9	*m*
	139	M
	176	M
10	71	*m*
	292	*m*
11	247	M
	156	M
	193	M
12	178	M
14	235	M
	47	*m*
15	22	M
	72	M
	103	*m*
	115	M
16	221	*m*
	99	M
	36	M
17	295	M
18	154	*m*
	151	M
19	54	M
20	227	*m*
21	298	M
	232	M

July (*con*).

d	No.	
22	248	*m*
25	199	*m*
	137	M
	230	M
	208	*m*
	240	M
26	134	*m*
27	166	M
28	71	M
29	103	M
	253	*m*

August.

d	No.	
1	251	*m*
	123	*m*
3	252	*m*
4	184	*m*
	183	*m*
5	163	M
7	221	M
8	131	M
	5	M
9	250	M
	41	*m*
	107	M
10	286	M
13	265	M
14	24	*m*
	249	M
	279	*m*
16	174	M
17	68	*m*
	287	M
18	105	M
	1	M
	26	M
	101	M
19	252	M
20	103	*m*
	81	*m*
	254	*m*
22	246	M
23	8	*m*
	291	M
24	35	M
	267	M
	19	M
25	71	*m*
	76	M
	126	*m*
26	18	*m*
28	125	M
	197	M
29	111	*m*
30	281	M
31	302	M
	182	M
	221	*m*

September.

d	No.	
1	258	M
2	92	M

September (*con.*)

d	No.	
2	88	M
	14	M
	103	M
3	135	M
	25	M
4	63	M
	240	*m*
5	127	*m*
6	244	*m*
	134	M
8	66	M
9	169	M
10	82	M
12	229	M
	71	M
	143	*m*
	239	M
	299	M
	138	M
14	274	*m*
	175	M
	39	M
	40	M
15	158	M
17	174	*m*
18	43	M
	216	*m*
	283	M
20	113	M
22	290	M
	228	M
23	221	M
	10	M
	177	M
	121	*m*
24	103	*m*
26	6	M
27	178	*m*
	146	*m*
28	187	M
	269	M

October.

d	No.	
1	240	M
	23	*m*
2	70	M
4	259	M
5	231	M
7	255	M
	148	*m*
8	151	*m*
	103	M
	125	*m*
	264	*m*
9	123	M
	253	M
10	71	*m*
	223	M
11	208	M
	17	*m*
	296	M
12	252	*m*
	15	M

October (*con.*).

d	No.	
12	280	M
13	214	M
14	136	*m*
	34	M
	118	M
15	46	M
17	221	*m*
	196	M
	28	M
	139	*m*
18	7	*m*
19	186	M
	161	M
	266	M
20	105	*m*
	67	M
	180	*m*
21	304	*m*
	62	*m*
22	250	*m*
23	168	*m*
24	30	M
25	68	M
26	134	*m*
	192	M
	147	*m*
	191	*m*
27	251	M
	4	*m*
	32	M
	297	M
28	163	*m*
	252	M
	71	M
29	181	M
	52	*m*
30	103	*m*
	194	M
	276	M
31	292	M
	154	M
	281	*m*

November.

d	No.	
1	164	M
	162	M
2	236	*m*
3	20	M
	268	M
	225	M
4	160	*m*
6	75	*m*
7	277	M
	149	M
8	221	M
9	19	*m*
11	47	M
	240	*m*
12	103	M
13	232	*m*
	24	M
14	300	M

November (*con.*).

d	No.	
14	171	M
	258	*m*
15	274	M
16	174	M
17	18	M
18	2	M
21	3	*m*
22	298	*m*
	100	*m*
23	230	*m*
	26	*m*
	55	*m*
	21	M
25	71	*m*
	189	*m*
26	122	*m*
	130	M
28	247	*m*
29	50	M
	112	*m*

December.

d	No.	
1	8	M
	260	*m*
2	275	M
	127	M
	221	*m*
4	282	M
	103	*m*
6	253	*m*
7	134	M
	29	*m*
	240	M
9	57	*m*
	58	*m*
11	305	M
12	102	*m*
13	71	M
14	12	M
15	43	*m*
16	35	*m*
	158	*m*
	139	M
17	301	M
	44	M
	53	M
	121	M
18	103	M
	212	M
19	248	M
	174	*m*
	252	*m*
	22	*m*
	81	M
	123	*m*
24	72	*m*
25	239	*m*
	221	M
	289	M
27	237	M
30	201	M
31	45	M
	57	

DOUBLE STARS.

No.	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch. 1890+.	Observe
		h m	° ′		°	″		
1.	Σ 3062	0 0·9	+57 52	6½ ; 7½	336·7	1·68	8·12	M.
2.	Σ 13	0 10·3	+76 24	6½ ; 7	87·7	0·92	5·90	C.
3.	OΣ 12, λ Cassiop.	0 26·4	+53 58	6½ ; 6½	148·6	0·58	4·03	B.
4.	OΣ 18	0 37·2	+ 3 36	7½ ; 9½	127·2	1·47	4·83	B.
5.	β 866	0 39·1	+42 34	9 ; 9	69·6	1·39	1·83	β.
6.	Σ 60, η Cassiop.	0 43·0	+57 17	3½ ; 7½	213·6	5·09	8·12	M.
7.	OΣ 20, 66 Piscium	0 49·2	+18 29	6 ; 6	330·2	0·42	7·88	Ls.
8.	Σ 73, 36 Andromedæ	0 50·1	+23 2	6 ; 6½	17·1	1·07	8·84	M.
9.	Σ 113, 42 Ceti	1 14·3	− 1 3	6½ ; 7	354·0	1·66	6·91	M.
10.	β 1000	1 30·3	−30 26	8 ; 13	356·4	1·44	1·84	β.
11.	Σ 138	1 30·8	+ 7 7	7 ; 7½	37·5	1·51	5·92	Ls.
12.	*p* Eridani	1 35·7	−56 0	6 ; 6	224·0	7·35	4·96	S.
13.	Σ 202, α Piscium	1 56·8	+ 2 17	4 ; 5	320·2	2·78	5·85	C.
14.	Σ 205, γ Androm. AB.	1 57·8	+41 51	2½ ; 5½	64·9	10·15	6·08	Ls.
15.	Σ 228	2 7·3	+56 59	6½ ; 7½	66·9	0·27	6·09	Ls.
16.	Σ 305	2 41·8	+18 58	7½ ; 8	317·2	3·02	8·11	Ls.
17.	Σ 333, ε Arietis	2 53·5	+20 56	5½ ; 6	202·1	1·34	6·08	Ls.
18.	β 525	2 53·5	+21 13	7½ ; 7½	131·0	0·27	8·01	Ls.
19.	β 530	3 9·1	+22 33	9½ ; 10	193·3	2·23	8·10	Ls.
20.	Σ 412, 7 Tauri	3 28·5	+24 8	6½ ; 7	204·5	0·30	6·08	Ls.
21.	Σ 422	3 31·8	+ 0 16	6 ; 8	247·1	6·19	5·90	C.
22.	Σ 425	3 33·8	+23 42	7½ ; 8	92·7	3·00	4·19	M.
23.	OΣ 531	4 0·5	+37 48	7 ; 7½	127·3	1·88	8·88	Ls.
24.	Σ 518, o^2 Eridani	4 10·6	− 7 49	8½ ; 10½	87·4	2·32	5·91	C.
25.	β 883	4 45·6	+10 54	7½ ; 8	41·5	0·26	8·09	Ls.
26.	β 552	4 46·1	+13 29	7 ; 10	189·9	0·46	8·02	Ls.
27.	Σ 535	4 17·7	+11 9	7 ; 8	330·5	2·07	5·06	Ls.
28.	Σ 572	4 32·2	+26 44	6 ; 6	202·1	3·69	4·13	M.
29.	OΣ 98, ι Orionis	5 2·4	+ 8 22	5½ ; 7	179·1	1·06	8·17	M.
30.	Σ 742, 380 Tauri	5 30·4	+21 56	7 ; 7½	258·8	3·50	5·16	M.
31.	Σ 749	5 30·9	+26 53	6½ ; 7½	174·7	1·12	8·21	M.
32.	Σ 932	6 28·5	+14 49	8 ; 8	328·1	2·34	3·18	Ls.
33.	OΣ 149	6 30·0	+27 22	7 ; 9	282·6	0·60	8·24	Ls.
34.	Σ 948, 12 Lyncis AB	6 37·3	+59 33	6 ; 6½	119·5	1·67	7·43	M.
35.	Sirius	6 40·6	−16 34	1 ; 10	175·9	3·92	7·73	Ait.
36.	Σ 982, 38 Geminor.	6 49·0	+13 18	5½ ; 8	160·7	6·77	3·21	Ls.
37.	Σ 1037	7 6·6	+27 24	7 ; 7	304·8	0·98	7·25	Mn.
38.	OΣ 170	7 12·2	+ 9 29	7 ; 7½	109·2	1·50	7·25	M.
39.	Σ 1066, δ Geminor.	7 14·1	+22 10	3 ; 8	205·3	6·89	6·08	C.
40.	Σ 1074	7 15·4	+ 0 36	7½ ; 8½	133·8	0·68	6·18	Ls.
41.	OΣ 175	7 27·7	+31 10	6 ; 6½	329·0	0·98	6·08	M.
42.	Σ 1110, Castor	7 28·2	+32 7	2 ; 3	228·1	5·96	7·14	Ls.
43.	Procyon	7 34·0	+ 5 29	1 ; 8½	326·5	4·83	8·21	Bd.
44.	Σ 1187	8 3·2	+32 31	7 ; 8	46·0	2·17	6·15	Ls.
45.	Σ 1196, ζ Cancri AB	8 6·4	+17 59	5½ ; 7½	18·1	1·03	6·18	Ls.
46.	„ „ AC	……	……	5½ ; 6½	116·3	5·22	6·18	Ls.
47.	„ „ BC	……	……	7½ ; 6½	128·9	5·50	6·18	Ls.
48.	β 205	8 28·8	−24 16	7 ; 7	244·4	0·81	7·23	Ait.
49.	β 208	8 34·6	−22 20	7 ; 8	52·3	0·70	2·11	β.
50.	Σ 1273, ε Hydræ	8 41·5	+ 6 50	4 ; 7	233·4	3·22	7·31	M.
51.	Σ 3121	9 11·9	+29 0	7½ ; 7½	198·6	0·83	8·29	M.
52.	Σ 1348	9 19·2	+ 6 50	7½ ; 7½	322·0	1·77	6·20	Ls.
53.	OΣ 201	9 18·0	+28 20	7 ; 11	222·4	1·24	7·31	Ls.
54.	Σ 1356, ω Leonis	9 23·1	+ 9 30	5½ ; 7	107·5	0·84	7·16	Ls.
55.	Σ 1389	9 45·3	+27 29	8 ; 9	309 5	2·48	6·20	Ls.
56.	A. C. 5, 8 Sextantis	9 47·5	− 7 37	5½ ; 6	93·6	0·42	96·88	Ait.
57.	OΣ 215, P. x. 23	10 10·9	+18 15	6½ ; 7	207·2	0·77	7·32	Ls.
58.	Σ 1424, γ Leonis	10 14·4	+20 22	2 ; 4	115·6	3·63	6·31	Ls.
59.	OΣ 218	10 22·1	+ 4 5	7 ; 9	74·0	1·04	6·30	Ls.
60.	OΣ 224	10 34·5	+ 9 22	7 ; 9	317·4	0·58	7·34	Ls.

DOUBLE STARS (*continued*).

No.	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch 1890+.	Observer.
		h m	° ′		°	″		
61.	OΣ 229	10 42·3	+41 38	6 ; 7	317·7	0·93	7·23	Ls.
62.	Σ 1523, ξ Ursæ Maj. .	11 12·9	+32 6	4 ; 5	163·8	1·94	7·31	Ls.
63.	Σ 1536, ι Leonis	11 18·7	+11 5	4½ ; 7½	59·6	2·53	6·34	Ls.
64.	OΣ 235	11 26·7	+61 38	5½ ; 7	98·1	0·91	7·38	D.
65.	β 603	11 53·5	+14 50	6½ ; 10	323·2	0·98	8·30	Ls.
66.	Σ 1647, 191 Virginis...	12 25·5	+10 16	7½ ; 8	224·4	1·00	7·34	Ls.
67.	Σ 1670, γ Virginis ...	12 36·7	− 0 54	3 ; 3	150·2	5·67	8·35	M.
68.	Σ 1687, 35 Comæ......	12 48·1	+21 48	5 ; 8	71·6	1·06	7·34	Ls.
69.	β 112	12 55·2	+19 1	9 ; 9½	292·0	2·24	6·41	Ls.
70.	OΣ 266	13 23·6	+16 14	7 ; 8	341·5	1·76	8·42	M.
71.	Σ 1757, P. xiii. 127 ...	13 29·3	+ 0 12	7½ ; 8	80·0	2·62	3·42	Ls.
72.	Σ 1768, 25 Can. Ven. .	13 33·2	+36 48	6 ; 8	138·8	1·41	7·45	D.
73.	Σ 1785	13 44·5	+27 29	7½ ; 8	270·5	1·57	8·44	M.
74.	Σ 1788	13 49·7	− 7 34	6½ ; 7½	75·5	2·60	6·38	M.
75.	Σ 1819	14 10·3	+ 3 36	7½ ; 7½	182·9	1·30	7·32	Ls.
76.	Σ 1865, ζ Boötis	14 36·3	+14 10	4 ; 4	352·4	0·10	8·44	Ls.
77.	Σ 1877, ε Boötis	14 40·6	+27 30	3 ; 6½	326·1	2·70	6·46	M.
78.	Σ 1879	14 41·3	+10 4	8 ; 8½	138·4	0·43	7·36	Ls.
79.	Σ 1888, ξ Boötis	14 46·7	+19 31	5 ; 7	213·0	2·88	8·48	M.
80.	β 31	14 47·8	+19 9	8½ ; 9½	194·4	1·36	7·44	Ls.
81.	OΣ 287	14 47·9	+45 20	7 ; 8	316·3	0·85	6·68	M.
82.	β 1086	15 2·2	+43 32	5½ ; 13	257·0	6·03	0·35	β.
83.	β 350	15 8·2	−27 10	7 ; 8½	160·2	1·15	2·41	L.
84.	Σ 1932, ι Cor. Bor....	15 14·0	+27 13	6½ ; 7	325·3	0·77	7·37	Ls.
85.	Σ 1937, η Cor. Bor....	15 19·1	+30 40	6 ; 6	329·1	0·40	7·34	Ls.
86.	Σ 1938, μ^2 Boötis......	15 20·7	+37 22	6½ ; 7½	82·2	0·92	6·56	M.
87.	OΣ 298	15 32·4	+40 8	7 ; 7½	178·0	0·90	8·53	Ls.
88.	Σ 1967, γ Cor. Bor....	15 38·5	+26 36	4 ; 7	121·5	0·40	7·38	Ls.
89.	OΣ 303	15 56·2	+13 33	7 ; 7½	141·6	0·67	7·43	Ls.
90.	Σ 1998, ξ Scorpii	15 58·8	−11 5	5 ; 5½	215·3	0·94	6·46	M.
91.	Σ 2032, σ Coronæ......	16 11·1	+34 7	6 ; 6½	210·6	4·16	6·45	Ls
92.	Σ 2055, λ Ophiuchi ...	16 25·8	+ 2 12	4½ ; 5½	47·0	1·40	6·44	Ls.
93.	Σ 2084, ζ Herculis ...	16 37·5	+31 47	3 ; 6½	288·2	0·67	8·64	Ls.
94.	Σ 2107, 167 Herculis .	16 47·8	+28 50	7 ; 8½	288·4	0·39	7·45	Ls.
95.	Σ 2140, α Herculis ...	17 10·1	+14 30	2½ ; 6	115·8	4·93	6·52	Ls.
96.	Σ 2173, 221 Ophiuchi	17 25·5	− 0 59	6 ; 6	335·1	1·08	7·54	M.
97.	OΣ 338	17 47·4	+15 21	6½ ; 6½	14·4	0·72	6·50	Ls.
98.	Σ 2262, τ Ophiuchi ...	17 57·6	− 8 11	5 ; 6	257·1	1·93	7·66	M.
99.	Σ 2272, 70 Ophiuchi .	18 0·4	+ 2 32	4 ; 6	290·2	2·08	6·62	Ls.
100.	OΣ 358	18 31·3	+16 54	6½ ; 7	193·5	1·85	8·62	M.
101.	β 648	18 54·2	+23 39	6 ; 9	231·2	1·16	7·44	Ls.
102.	γ Coronæ Aust..........	19 0·0	−37 12	5½ ; 5½	164·0	1·03	4·71	S.
103.	β 248	19 13·4	+22 51	6 ; 9	129·6	1·84	6·86	M.
104.	Σ 2579, δ Cygni	19 41·8	+44 53	3 ; 8	303·4	1·50	7·53	M.
105.	Σ 2695	20 27·7	+25 28	6 ; 8	78·5	0·96	8·71	M.
106.	β 151, β Delphini ...	20 32·7	+14 15	3½ ; 5½	349·7	0·68	5·38	Bd.
107.	Σ 2729	20 46·3	− 5 59	6½ ; 6½	191·8	0·20	5·81	Ls.
108.	Σ 2799	21 23·9	+10 38	7 ; 7½	119·1	1·47	8·83	M.
109.	Σ 2822, μ Cygni	21 39·7	+28 18	4 ; 5	121·5	2·39	7·79	M.
110.	Σ 2909, ζ Aquarii......	22 23·7	− 0 32	4 ; 4	322·7	3·31	6·86	M.
111.	Σ 2944, AB	22 42·7	− 4 45	7 ; 7½	255·5	3·50	2·76	M.
112.	OΣ 483, 52 Pegasi......	22 54·2	+11 12	6 ; 7½	222·7	1·15	8·94	M.
113.	β 79	23 11·8	− 2 9	8 ; 8½	90·6	1·01	4·96	Bd.
114.	β 80	23 13·2	+ 4 53	8 ; 8½	325·1	0·64	4·85	Ls.
115.	β 720	23 29·0	+30 46	6 ; 6	158·1	0·50	8·73	Ls.

In the column headed "Observer," the following abbreviations are used:—

L. Leavenworth.	B. Bijourdan.	Ait. Aitken.	C. Collins.
Ls. Lewis.	D. Doolittle.	Mn. Morgan.	Bd. Barnard.
β. Burnham.	M. Maw.	S. Sellors.	

THE OBSERVATORY,

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, December 9, 1898.

Sir R. S. Ball, LL.D., F.R.S., *President*, in the Chair.

Secretaries: Prof. H. H. Turner, M.A., B.Sc., F.R.S., and H. F. Newall, M.A.

The Minutes of the previous Meeting were read and confirmed.

The President. Since the last Meeting the Society and the astronomical world have sustained a serious loss by the death of Mr. Edwin Dunkin. Mr. Dunkin was for many years a greatly esteemed and distinguished Fellow of this Society, filling in turn, with equal capability, most of the offices. Accordingly I propose the following resolution of condolence:—

> That this Society, having heard with deep regret of the death of Mr. Edwin Dunkin (for 53 years a greatly esteemed and distinguished Fellow of this Society, who was, moreover, for many years on the Council, filled the Offices of Secretary, Vice-President, and President), desire to put on record their profound sorrow at the loss which Astronomy has sustained, and to express their condolence and deep sympathy with Mrs. Dunkin and the other members of the bereaved family; and that a copy of this resolution be sent to Mrs. Dunkin.

The resolution was agreed to.

Mr. Newall. Sixty presents have been received since the last Meeting. Among them is a series of photographs of the Sun, original negatives, presented by Mr. Newbegin. From Prof. André we have received Part I. of his treatise on Stellar Astronomy. We have also had sent us the seventh volume of the 'Annals of the Bonn Observatory,' containing, among other things, Argelander's observations of variable stars. Two books have been given us by Prof. Meldola—an early edition of Coddington's 'Elementary Treatise on Optics' and a book by Dr. Kitchener on the 'Economy of the Eyes.' The eighth volume of the new edition of the Works of Galileo has been presented to us by the Italian Government, and we have also received Vol. I. of the 'Annals of the Lowell Observatory,' containing observations of Mars during the opposition 1894–5, and the Report of the Madras Observatory for the years 1895 to 1898, which gives also the results of the eclipse expedition of 1898 January. Also a work by Prof. Schiaparelli relating to the origin of the heliocentric planetary system amongst the Greeks. As to serial publications, of which we have so many in our library, I may mention that the Companion to the 'Observatory' is published this month, and that the Bureau of Navigation at Washington has recently published vol. vi. part 4 of the 'Papers of the American Nautical Almanac Office,' which consists of tables of the heliocentric motion of Mars by Prof. Newcomb.

A vote of thanks was accorded to the donors of the presents.

On the motion of *Mr. W. E. Plummer*, seconded by *Mr. Franklin Adams*, Mr. Levander, Mr. T. Lewis, and Mr. Inwards were elected Auditors of the Treasurer's accounts for the year 1898.

Mr. Newbegin showed on the screen his series of photographs of sun-spots, calling attention to the solar activity, excessive considering the present phase of the sun-spot cycle, which happened in September and October, and lasted even up to November 10.

Capt. W. Noble. I was not able to be in my observatory to examine this group of spots in the early days of their existence, but I have lately seen a curious pear-shaped group of spots, which I think will repay examination by any one who is favoured with fine weather.

Mr. Seabroke. It would be interesting to know how these photographs were taken, whether they were taken direct from the object-glass or whether they are magnified in the telescope. They are very excellent specimens of photography, and I think Mr. Newbegin is to be congratulated upon the success which has attended his efforts to obtain this series of photographs during a time when the weather was very bad generally.

The President. We are very much obliged to Mr. Newbegin for showing us these very interesting photographs. May I ask him to favour us further with the information asked for?

Mr. Newbegin. The photographs were all taken with a Cooke triple-lens objective of 4¼ inches aperture stopped down to 2½ inches. Exposure of $\frac{1}{100}$ second is given by means of a Thornton-Pickard focal plane shutter. The camera with its Barlow lens magnifies the image four times, and the picture which results is within the limits of size of a lantern-slide and can be thrown upon the screen. With reference to Capt. Noble's remark, there is a very interesting sun-spot now on the Sun, double in form, and of which one component is very much of a pear-shape formation. It is now past the meridian. I had the pleasure of getting photographs to-day and yesterday, with both of which

I am satisfied, and I hope on a future occasion to ask the Fellows to look at these. The plate I used is the Ilford ordinary, and the focal length of the telescope about 71 inches.

Prof. Turner partly read a paper by *Dr. Gill* on a Micrometer for Measuring Astrographic Plates. Dr. Gill said that having examined the methods in use at Greenwich and at Oxford, he could not satisfy himself that results of adequate accuracy could be obtained by such means, so that the method he proposed to employ essentially differed from these, as he intended to use micrometer-screws instead of scales. Prof. Turner said that at Oxford he had adopted scales as being much more rapid to use. There was an objection to the use of screws in that they were liable to wear, and by an odd coincidence a paper had been received from Dr. Gill for this Meeting on the wear of micrometer-screws. The machine described by Dr. Gill was a beautful instrument, and was a credit to the great skill of Messrs. Repsold. It was rather expensive, as it cost about £180, six times the cost of those in use at Oxford.

Prof. Turner then read a paper by *Dr. Gill* on the Effect of Wear on Micrometer-screws. Some years ago Mr. Christie called attention to the serious effect of the wear of screws, and he found that the star-places made at Greenwich were subject to serious errors from this cause. Dr. Gill suggested the use of steel instead of gun-metal as material for screws. It had been suggested at Greenwich that one way of getting over the difficulty would be to reverse three of the screws, so that the effect of the wear would be in opposite directions. Dr. Gill appears to have followed this plan, and though the steel screws had certainly worn, the effect of the wear had been practically eliminated by reversing three of the screws as opposed to the other three. (A statement of the errors due to screw wear was given on the board.)

Mr. Knobel. Are there errors in the observations due to the wear of the Cape screws?

Prof. Turner. Yes; but the errors as shown in the first column are too small to apply.

Mr. Newall. It shows that the substitution of steel for gun-metal gives a great improvement.

Prof. Turner. Yes; but it is not a complete solution of the difficulty.

Mr. Dyson. The experience at Greenwich has been very similar. A reversal of three of the screws was made in 1886; the screws were examined in 1893 and 1896, and the same thing was shown as is now pointed out by Dr. Gill—namely, that the reversal of the three screws almost entirely eliminated the errors, whereas if they had not been reversed there would have been an error of about the same amount as Dr. Gill gives. At Greenwich in 1896 the telescope micrometer-screw was also found to have worn considerably. Over a range of about ten revolutions the screw seemed to have worn by an amount equal to quite 1″. The screws are being examined again now, and it seems that they have worn 0″·2 or 0″·3 in their whole range. These facts show how careful one ought to be with screws, and that they should not be used to make thousands of measures without continual examination.

Mr. Inwards. I do not know whether the steel screws work in steel nuts or in gun-metal nuts, but I am sure that gun-metal nuts cut screws very much more than hard steel nuts. Steel on steel wears slower than steel on gun-metal.

Capt. Noble. In using a micrometer for measuring ordinary small distances one uses nearly the same part of the screw all the time, unless a large shift is purposely given.

Prof. Turner. The illustration that Dr. Gill gave of this point some years ago was this:—Suppose the thread of the screw be unwrapped so that it forms the inclined surface of an inclined plane. A nut working on the same part of the screw will cause a hollow in this inclined surface; the sides of this hollow will have inclinations differing from one another and from the general inclination of the plane.

Mr. Maw. What length of nut is used on the screws, and were the screws examined by a microscope to see how they wear?

Mr. Dyson. We have not so examined the screws used at Greenwich.

Mr. Dyson (giving the heads of a paper presented by himself and *Mr. Thackeray*). The systematic differences between the circumpolar observations at Greenwich referred to in the Astronomer Royal's Report to the Board of Visitors in June, 1898, led us to re-examine the division-errors. These have been re-determined, and previous determinations were compared and combined with them. The method of determination is given in detail in the paper, and it is found that a much more satisfactory agreement in the circumpolars is arrived at by use of the new division-errors, and also a better agreement between the Greenwich 1880 Catalogue and the Cape 1885. The new division-errors so far reduce these discordances that there is no reason to suppose that their cause is to be sought in the temperature of the observing-room. Further, the question of what refraction best suits the Cape and Greenwich is briefly considered in this paper.

Mr. Knobel. I understand that these division-errors are applied to observations of certain circumpolar stars. May I ask whether the observations made to the north of the zenith appear to require a refraction-correction similar to that to be applied to those in the south? That is a curious point which was determined by Mr. Johnson when he was in St. Helena. To the north of Greenwich there is marsh land and the river, and on the south there is not. I should like to ask Mr. Dyson whether he suspects any difference in refraction due to those causes.

Mr. Thackeray. There is nothing to lead us to suspect that the refraction differs north and south of the zenith. It is an exceedingly difficult matter to determine what is the temperature of the air for purposes of refraction. For many years the thermometer used in the reductions of the Transit-Circle observations was mounted close to the north wall of the Transit-Circle room; careful comparison of this thermometer with the Meteorological Standard thermometer showed that during the daytime its readings were systematically too low; that at present in use is mounted on the north side of the front court, with a more free exposure. It would appear that at night-time there is little or no difference between thermometers placed in different positions, so that the value of co-latitude derived from the stars is scarcely affected by the position of the thermometer, whereas during the day, especially about sunrise, noon, or sunset, the reading of a thermometer is liable to considerable systematic differences depending on the conditions of its exposure. From a discussion in this paper of the Sun-observations for the last 20 years it would appear that, using Bessel's refractions and the Meteorological Standard thermometer, the results from the Sun and stars agree; but if we adopt the Poulkova refractions there is developed a discordance of about 0″·5 between them, which would be diminished by one half if the old thermometer, mounted close to the Transit-Circle room building, and whose readings were found to be systematically low owing to faulty exposure, were to be used for the reductions, and would be entirely removed if a position for this thermometer could be found which would make it read at noon as much lower as the Meteorological thermometer now reads higher.

Capt. W. Noble. Mr. Dyson has told us that these corrections diminish the discordance between the observations of circumpolar stars from which the co-latitude is determined, and that the value of the co-latitude has altered. Has that any bearing upon this doctrine of the rotation of the Earth's axis of figure about the axis of rotation?

Mr. Dyson. No.

Prof. Turner. With reference to Mr. Knobel's question as to whether refraction is the same north and south of the zenith, we may remark how little we know about refraction. We do not know what refraction is in azimuths other than north and south, not that there is any difficulty, but no experiments have been made on the point. That is one of the things that the Astronomer Royal hopes to investigate with the new altazimuth. As regards the question of north and south, we observe stars north and south on the meridian, but when we try to compare these with other results we are always finding some discrepancy which may be due to flexure of the telescope, or to the heat of the air in the room, or to some other cause which may be assigned as a reason for the discrepancy. I congratulate Mr. Dyson and Mr. Thackeray on having found another of these small errors, which is only one of the important things in their paper. It is only by persistent and very careful work of this kind that we can avoid having to make new hypotheses every day, which is a serious thing in retarding astronomical research.

Mr. Newall. I am reminded of a point I have read about during the last few weeks relating to some observations by Prof. Comstock on the zenith-distance of stars at different times of the year, in which he appears to have traced the evil effect produced by the observatory chimney. I think I am right in saying that he attributes to the simple fact of the proximity of the chimney to his telescope a discrepancy in his observations of about 1″.

Mr. Lindemann. On this point I may say that at my observatory observations in the eastern sky during the evening are unsatisfactory, whereas in the west they are good, and I have not yet found out the reason of this. There are double stars which I cannot separate when they are in the east, but I can separate them easily when in the west. I find that I cannot work satisfactorily before half-past eleven or twelve o'clock at night.

The President. We are much obliged to Mr. Dyson and to Mr. Thackeray. The smallness of the probable error of the new value of the co-latitude is astonishing. Every one thought it was a great achievement when Nansen got within 250 miles of the Pole; but if the Pole will shift its position only by the width of this table it will be appreciated at the Greenwich Observatory.

Mr. Plummer presented a paper on Comets, and in answer to the President proceeded to make a statement on recent work at the Liverpool Observatory. I wish to thank the Fellows for the kindness with which they have received my name, and it may possibly enable me to offer some remarks as to what we are doing at Liverpool in the way of advancing astronomical science. There must be two kinds of observatories—the observatory for research and the observatory for use. The Liverpool Observatory is one for use. The determination and distribution of time and the examination of instruments for use are not very interesting, but they are necessary. I do not know what view this Society takes of the projected increase of the Kew Observatory. This is a matter which interests us at Liverpool, and I will ask Fellows of the Society to consider it, and so far as they can to forward the movement, which has for its aim the distribution of more accurate instruments. By the kindness of the British Association I have mounted in one portion of the observatory one of their seismometers, its records have given me considerable anxiety, but they may not be without some interest to the members of the Society who work at the astronomy of accurate position. The trouble that I have is that the angle through which the needle of the instrument moves varies from day to day, and by no small amounts. I have records which correspond to a variation of level of 0″·1 during the day; and in accurate determinations of zenith-distance it might be necessary to consider this diurnal variation of the position of the nadir. The geological formation of the hill on which the observatory stands is not very suitable, and this may supply a cause for this shifting in level *.

Prof. R. L. Jones (Madras) then gave an account of the eclipse of 1898. He said:—I have no communication to make specially with regard to the eclipse, but I have been asked to show some slides. These slides will show what results were obtained by the Madras Observatory party at Sahdol, near the station occupied by the Astronomer Royal and Professor Turner. The work undertaken by the Madras Survey party was divided into four parts—first, to take a series of photographs with the 40-foot telescope; the second to take a series of photographs with the photo-heliograph of 4 inches aperture and 5 feet focal length; the third party used a small spectrograph, and the fourth a polariscope. There is not much that I can say about the last two, but I will show the results obtained with the 40-foot telescope and with the photo-heliograph. (Screen.)

Capt. von Geyzel then gave a description of the manner in which the plates were developed.

Prof. Turner. I was camped near the party, and I can only say that we had immense admiration for the way in which Mr. Michie Smith and those who were with him worked under difficult conditions, and nobody was more pleased than ourselves when we found that they had obtained such results. Their camp was pitched near to ours, and we compared a good many notes. I must say, however, that we were a little envious of them, because when a shooting expedition was organized Mr. Michie Smith saw the tiger and we did not. (Laughter.)

The President. I am sure we are all very pleased to hear what has been said upon this subject; and with regard to Prof. Turner's last remark, namely, that he did not see the tiger, I think we all have reason to congratulate ourselves upon the fact that the tiger did not see him. (Laughter.)

Capt. Taylor. I want to ask if anyone has had any experience of the astronomical conditions of the West Coast of Africa? I am going up the Niger shortly, and if I can further the astronomical cause in that region I shall be happy to do so. I have never been in that part of the world, and therefore I should like to have some information regarding the climatic conditions with a view to astronomical research.

The President. As there is no response it does not appear that anybody is bursting with information to give to Capt. Taylor; but if any such should reach us, I think I may undertake, on the part of the Secretary, that it shall be sent to him.

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* See p. 55.

The following papers were announced and partly read:—

F. W. Dyson and W. G. Thackeray. "The Division-errors of the Greenwich Transit-circle."

David Gill. "On a new Instrument for Measuring Astrophotographic Plates."

A. R. Hinks. "Observations of the Leonids, 1898 November, made at Cambridge Observatory."

David Gill. "Note on the Effect of Wear on the Errors of Micrometer-screws."

W. F. Denning. "On a Probable Instance of Periodically Recurrent Disturbance on the Surface of Jupiter."

John Tebbutt. "Observations of Comet Coddington (*c* 1898)."

Rev. T. E. R. Phillips. "The Extra-Equatorial Currents of Jupiter during the Apparition of 1897–98."

W. R. Brooks. "Discovery of Comet Brooks 1898."

Royal Observatory, Edinburgh. "The November Meteors."

W. E. Plummer. "Cometary Observations at the Liverpool Observatory, 1897–98."

A. C. D. Crommelin. "Ephemeris for Physical Observations of the Moon in the first half of 1899."

Royal Observatory, Greenwich. "Observations of Comet *i* 1898 (Brooks)."

"Observations of the Leonid Meteors, 1898 November." (Communicated by Dr. G. Johnstone Stoney.)

The following gentlemen were elected Fellows of the Society:—

Lieut. Tristan Dannreuther, R.N., F.R.G.S., H.M.S. 'Leander,' Pacific Station, Esquimault, British Columbia.

Charles Thomas Whitmell, M.A., B.Sc., Invermay, Headingley, Leeds.

The following gentlemen were elected Associates of the Society:—

O. Backlund, Director of the Observatory, Poulkova, Russia.

Edward Emerson Barnard, D.Sc., F.R.A.S., Yerkes Observatory, Williams Bay, Wisconsin, U.S.A.

Sherburne Wesley Burnham, M.A., F.R.A.S., Government Building, Chicago, Ill., and Yerkes Observatory, U.S.A.

Commandant G. Defforges, Service Géographique de l'Armée, Paris.

James Edward Keeler, D.Sc., Director of the Lick Observatory, California, U.S.A.

Henry A. Rowland, Johns Hopkins University, Baltimore, Md., U.S.A.

Wilhelm Schur, Director of the Observatory, Göttingen, Germany.

The following Candidates were proposed for election as Fellows of the Society:—

M. E. M. Antoniadi, Observatory, Juvisy, Seine-et-Oise, France (proposed by Capt. W. Noble).

John Jepson Atkinson, Barrister-at-Law, Cosgrove Priory, Stony Stratford (proposed by Dr. A. A. Common).

Dr. W. Lee Dickinson, 9 Chesterfield Street, Mayfair, W. (proposed by Dr. E. J. Spitta).

John James Hall, Observatory Cottage, Datchet Road, Slough (proposed by W. H. Walmsley).

Joseph Larmor, M.A., D.Sc., F.R.S., St. John's College, Cambridge (proposed by H. F. Newall).

John R. Reynolds, Malvern House, Trinity Road, Birchfield, Birmingham (proposed by Sir J. B. Stone).

Charles Almeric Rumsey, B.A., Master at Dulwich College, London, S.E. (proposed by E. T. Whittaker).

Charles Stevens, Civil Servant, 10 Wemyss Road, Blackheath, S.E. (proposed by Percy L. H. Davis).

William Harold Tingey, B.A., F.R.Met.Soc., Rede Court, Rochester, Kent (proposed by Harold I. Adams).

Thomas Weir, Sec. North-western Branch of the British Astronomical Association, 56 Parkfield Street, Moss Lane East, Manchester (proposed by E. W. Maunder).

Algernon Charles Legge Wilkinson, B.A., Trinity College, Cambridge (proposed by E. T. Whittaker).

THE BRITISH ASTRONOMICAL ASSOCIATION.

THE first ordinary Meeting of the ninth Session of the British Astronomical Association was held at Sion College on Wednesday evening, November 30, *Mr. W. H. Maw*, President, in the Chair.

The election of fourteen new Members was confirmed, and the names of eight Candidates were read and suspended.

Mr. Goodacre, Director of the Lunar Section, reminded the Meeting that there would be a total eclipse of the Moon on December 27, and urged as many Members as possible to take part in observing the phenomena. Mr. Elger, he said, had on a previous occasion suggested that special attention should be devoted to the following points, and he could not do better than repeat them:—(1) At what stage of the eclipse is the penumbra first visible to the naked eye and in the telescope? (2) Its apparent density; whether the Earth's shadow is preceded or followed by any colour, or any degradation of the Moon's brightness. (3) The definition of the boundary of the Earth's shadow—whether sharp and well defined, or exhibiting the "shading off" which has occasionally been remarked; whether it is everywhere circular, or displaying local irregularities similar to the elevation or cusp noted by M. Boë during the eclipse of October 4, 1884. (4) The colour and density of the shadow in various parts of the disk should be noted at frequent intervals. (5) Record the times when the shadow reaches different lunar formations, and note its effects on their visibility during the progress of the eclipse.

Mr. Crommelin said that one great value of a lunar eclipse, quite apart from the question of lunar physics with which Mr. Goodacre had dealt, was the opportunity it gave for improving their knowledge of the size and distance of the Moon by observing the occultations of the faint stars behind the Moon at the time. Dr. Backlund, Director of the Poulkova Observatory, had prepared from photographs of the region a very full list of the places of all the small stars in the neighbourhood, and had computed the circumstances of their disappearance and reappearance for something like 150 observatories. As the approaching eclipse would be visible over the whole of Europe and Africa and a great part of Asia and America, they ought with favourable weather to improve their knowledge of the size and distance of the Moon. Of course these were known within very narrow limits, but there was room for still greater accuracy.

Mr. Crommelin then presented a report (accompanied by diagrams) on the observations of Brooks's Comet by Members of the Cometary Section. He stated that the comet was a very conspicuous object in the evening sky during the first half of November. Its apparent motion was very rapid, as the Earth and the comet were moving in opposite directions. It might be seen again in the Southern Hemisphere in January and February, but it would be very faint. Mr. Crommelin also mentioned the discovery of a new faint comet by Mr. Chase at Newhaven, Conn., on November 14.

Mr. Newbegin showed on the screen fifteen photographs taken from September 10 to October 9 this year, illustrating an almost continuous display of solar activity in both hemispheres.

Mr. Maunder thought Mr. Newbegin's series of photographs very interesting; he was specially pleased to see the notch on the limb when the great spot was going off on September 15. In the Greenwich photographs the same notch was shown, but not so distinctly. The sun-spots had been more than usually interesting during the past two years. As was well known to all solar observers, the spots had a tendency to run down in solar latitude from the time of minimum. When there were very few spots on the Sun at dead minimum, they broke out in high latitude, say 30° or even 40° from the equator. Then as spots increased they got them mostly about 15° on either side. In the present cycle, for something like four years the mean distance from the equator did not vary much, remaining about 14° or 15°, but in 1897 a very curious phenomenon took place. In the first half of the year the northern spots ran down very quickly in latitude and nearly died out about latitude 2°, and then about the middle of the year there were some fresh outbursts, the spots at the same time lifting in latitude very considerably; but 1897, as a whole, showed a considerably lower latitude than the four preceding years, viz. 8° as against 14°.

Capt. Steele expressed the opinion that the spots on the Sun's disk must be due to disturbances in the Sun's atmosphere rather than formations in the Sun itself.

Mr. Offord then showed some lunar photographs that he had recently taken.

The President congratulated both Mr. Newbegin and Mr. Offord on their success, and expressed his pleasure that so many Members were doing really good work in astronomical photography.

Miss Orr contributed a paper (read by *Mr. Schooling*) on "Black Star-Lore." The writer referred to the valuable work being done in Australia by Mrs. Langloh Parker in this department. Mrs. Parker had found that quite a number of stars were known to the blacks, who had several very interesting and suggestive legends about them. Miss Orr concluded by asking "Do these fairy tales seem frivolous to a learned society? Or is it true that the beliefs and fancies of a savage race fast dying out are worthy of study and investigation?"

Mr. Schooling said that for himself he regarded these early astronomical legends as the most interesting things they could have before them. The history of astronomical ideas, if we could only trace them from the earliest times, would form a very fascinating study, and would, no doubt, illustrate on the mental side a process of evolution corresponding to the physical evolution which modern astronomy disclosed.

Mr. Maunder remarked that they were greatly indebted to Miss Orr for her interesting paper. Seeing that the Australian aborigines had been, for the greater part of their existence as a nation, separated from the influences of more civilized nations, genuine Australian legends would be of the greatest value; but great care would be required in collecting them to be sure that they were the genuine products of the native mind and not native renderings of what they had heard from Europeans.

Mr. Schooling read a paper from *Herr Leo Brenner* on "The Impossibility of the Martian Hypothesis of Mr. Lowell." The paper dealt with numerous points in Mr. Lowell's book and contended that none of them were verified.

Mr. Maunder referred to the recently published 'Annals' of the Lowell Observatory, and remarked that many of the observations were simply startling in the refinement and accuracy of detail claimed for them. They would not be out of the way if Mars had been apparently as large as the Moon.

The speaker then referred with approval to a suggestion of the late Mr. Marth that observers should try to draw the Moon either with the naked eye or with a power of two or three diameters, because the Moon would then appear to them very much as Mars did with ordinary magnification.

Mr. L. B. Tappenden thought, with regard to observations of Mars, that a comparison of such observations taken at different dates would be facilitated if the Martian month were given.

Mr. Crommelin thought that one of Herr Brenner's criticisms of Mr. Lowell's work was unfair. Herr Brenner appeared to expect that an exact repetition of the phenomena on Mars, noted by Mr. Lowell in 1894, should take place in the following Martian year. Considering how enormously different years varied with regard to all weather phenomenon on the Earth, this was not a reasonable expectation.

In reply to Mr. Ellis, Mr. Maunder said that he did not think either Herr Brenner or Mr. Lowell had tried photography.

A paper on "Observations of the Zodiacal Light,' by *Capt. P. B. Molesworth*, was then read, Mr. Maunder adding that in a private letter to him Capt. Molesworth told him that after several unsuccessful attempts he had definitely succeeded in photographing the Zodiacal Light.

In a paper on "Dark Meteors," *Mr. T. W. Craven* described some experiments of his which led him to the conclusion that in some cases, at any rate, the so-called dark meteors were minute particles of the dead black coating inside the eyepiece of the instrument, which, becoming detached, had fallen more or less vertically.

ROYAL METEOROLOGICAL SOCIETY.

THE monthly Meeting of this Society was held on Wednesday evening, December 21, at the Institution of Civil Engineers, Great George Street, Westminster, *Mr. F. C. Bayard, LL.M.*, President, in the Chair.

Capt. A. Carpenter, R.N., D.S.O., F.R.Met.Soc., gave an account of the hurricane which caused so much devastation in the West Indies in September last. The cyclone, passing 18 miles south of Barbados, swept over the southern half of St. Vincent Island, then took a north-west direction towards Aves Island, its rate of progression being about 7½ miles per hour. From here it pursued a northerly course for 450 miles, passing between Puerto Rico and the Windward Islands. It then swerved to the north-west for 600 miles, when it recurved to the north-east. Its diameter was 80 miles as it approached Barbados, and 170 miles after leaving St. Vincent. The actual storm-centre (in which the force of the wind greatly increased) was only 35 miles in diameter, until St. Vincent was passed; but after that the strength of the wind extended to 170 miles from its centre. The diameter of the calm vortex, or "eye" of the storm, was not less than 4 miles. The storm was accompanied by very heavy rainfall, the amount at St. Vincent being about 14 inches in the 24 hours ending at 9 A.M. on the 12th. The barometer at the Botanic Gardens, Kingstown, on the 11th fell from 29·539 ins. at 10 A.M. to 28·509 ins. at 11·40 A.M., a fall of 1·03 in. in 1 hour 40 mins. In Barbados 11,400 houses were swept away or blown down, and 115 lives were lost; and in St. Vincent 6000 houses were blown down or damaged beyond repair, and 200 lives lost.

Mr. W. H. Dines, B.A., F.R.Met.Soc., read a paper on the connection between the winter temperature and the height of the barometer in North-western Europe. From an examination of the records of the barometer and temperature at several observatories extending over many years, the author is of opinion that the winter temperature at a place in Western Europe has no connection with the height of the barometer at that place, and that in winter it is just as likely to be cold when the barometer is below the average, as when it is above the average.

Edwin Dunkin, F.R.S.

THE record of the life of the veteran astronomer whose memory was honoured by a special resolution at the last meeting of the Royal Astronomical Society may be summed up by saying that it was that of a man who found his life's happiness in faithfully doing his duty. Mr. Dunkin was not a great astronomer whose name will be quoted with those of Herschel, Airy, or Adams; his work in his own branch, though less brilliant, was not less arduous nor less useful, but equally deserving of renown with that of those celebrated men.

We have record of Mr. Dunkin's life from its earliest moment, for his birth is mentioned in the Minute-book of the Board of Longitude. His father, William Dunkin, who lived at Truro, Cornwall, was a computer on the staff of the 'Nautical Almanac,' and had occasion about this time to write to the Secretary of the Board, and in the letter* mentioned the birth of a son on 1821, August 19.

This son, Edwin, the third of the family, was educated at private schools in Truro until 1832, when, on the establishment of the 'Nautical Almanac' Office in Somerset House, the family removed to Camden Town, London, and the lad was sent to schools in that neighbourhood—first to Grove House, and then to Wellington House, Hampstead Road, where Charles Dickens had been a pupil a year or two before. In 1837 he was sent to a school at Guines, near Calais, where he remained until the death of his father in the summer of 1838.

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* Observatory, vol. xxi. p. 123 footnote.

Having now to choose his career in life (his two elder brothers had pre-deceased their father), and being inclined to science, Edwin Dunkin naturally turned to his father's old friend, Davies Gilbert, F.R.S., for advice, and to Lieut. Stratford, the Superintendent of the 'Nautical Almanac,' for help. Through their influence he and his younger brother Richard were chosen by Airy as two of a band of young men he was then gathering together, quite distinct from the staff of the Royal Observatory, to effect the reductions of the planetary and lunar observations of Bradley, Bliss, Maskelyne, and Pond. The severity of this work has often been mentioned by Mr. Dunkin: the lads worked from eight in the morning until eight at night, with only one hour's interval, and were kept under the strictest supervision, so that it was a welcome relief when in 1840 he, with J. R. Hind, was attached as computer to the newly formed Magnetic and Meteorological Department at Greenwich under Mr. James Glaisher. The hours in this department were then irksome, for as this was before the era of photographic registration, eye-observations of the magnetic needles had to be made every two hours throughout the day and night. In October 1845 Dunkin was transferred to the Astronomical Department, and took his turn as a regular observer with the meridian instruments. It is worthy of mention as evidence of Mr. Dunkin's character, and the pride he took in his work, that he has more than once said to the writer of this that one of the pleasantest moments of his life was when he first saw his name printed in the Greenwich volume, and his happiest period was when, as a young man, he followed the regular and healthy life of an observer, taking his turn for duty on alternate days, in friendly emulation with his colleagues as to who could do the best day's work. He must have early obtained some reputation, for when H.M.S. 'Beagle' was setting forth on her scientific expedition, and her crew of observers was being chosen, Dunkin was given the opportunity of being the astronomer of the party; but as he had then met the lady who was to be his life-partner, and was looking forward to marriage, he was loth to leave. Airy, too, must have had a high opinion of his zealous assistant, for when the altazimuth was set up in 1847, he put Mr. Dunkin in complete charge of the instrument and of the intricate reductions: he and one computer, Hugh Breen, for several years alone used this instrument; they took turns of half a lunation in length and observed the Moon every night. The severity of this arrangement can be easily realized. When Airy organized the Harton Colliery experiment in 1854 Dunkin was one of the observers. He was one of the earliest astronomers who made use of the telegraph for the determination of terrestrial longitude, for he and M. Faye of Paris were the representatives of the two National Observatories in the first measurement of the arc, Greenwich—Paris, by means of the telegraph. An examination of the details of this work will show Mr. Dunkin's quality as an observer. He was the chief observer in the telegraphic determination of the longitude of Valentia in 1862.

Mr. Dunkin joined the Royal Astronomical Society in March 1845, and in January 1852 made his first contribution to the 'Monthly Notices,' on the subject of the solar eclipse of 1851 July, which he had observed from Christiania, whither he had accompanied the Astronomer Royal's expedition. The interest of a solar eclipse at that time lay chiefly in the study of prominences and of the phenomenon known as Baily's Beads. It is curious to note now that Mr. Dunkin gave it as his opinion, though with reservation, that the prominences *had some connexion with the Moon.* Five years later he contributed his second paper, "On an Occultation of Jupiter," and from that time forward papers by him were fairly frequent, generally on subjects immediately connected with his work, on observations of occasional phenomena or on probable error and personality of observation. His most important work, which was no doubt instrumental in gaining for him Fellowship of the Royal Society, was a discussion of the Solar Motion in Space from the proper motions of 1167 stars*, the result of which was closely similar to that found by Herschel eighty years earlier. He became a member of the Council of the Royal Astronomical Society in 1868, and in February 1870 he was elected Secretary. He filled this office in a most capable manner until February 1877. Under his management the February number, which, as a report of the science during the previous year, forms such a useful contribution to astronomical literature, grew to its present size. Necessity occurred during his tenure of office to publish obituary notices of several distinguished astronomers, and several of these from his pen have been published in one interesting volume. Whilst he was Secretary, the Society changed its quarters from Somerset House to its present home in Piccadilly, and the management of this removal and the furnishing and arrangement of the new quarters in Burlington House were effected almost entirely by Mr. Dunkin. In 1884 he was elected President of the Society, and occupied the chair for the usual period of two years. In June 1876 he was elected a Fellow of the Royal Society, and served on the Council for two years, 1879–81.

In 1870 Mr. Dunkin was relieved from the work of observing at Greenwich, and was made Superintendent of Computers, which office carried with it the control of all the reductions relating to observations with the Transit-Circle. The retirement of Sir George Airy in August 1881, and the appointment of Mr. W. H. M. Christie to the office of Astronomer Royal, left the post of Chief Assistant vacant, and Mr. Dunkin was chosen to fill this for a defined period of three years, dating from 1881, Aug. 25, so that he ended his official career on 1884, Aug. 25, at the age of 63, after a service of 46 years. Since then he has taken little active part in astronomy; he attended the meetings of the Society occasionally, but not frequently, as the journey from town late at night was prejudicial to his health. He was almost always present at the Visitation of the Royal Observatory and similar functions, when it was pleasant to see the delight with which he renewed acquaintance with the instruments so well known to him, and discoursed to his former colleagues on his earlier experiences. He tolerated the introduction of the Sun-spot department, and also of the spectroscope, as these did not compete in any way with the Transit-Circle; but he, with conservative mind, could never be brought to appreciate the application of photography to the determination of star-places, which he seemed to consider cast some derogation on old and well-tried methods. He died at "Kenwyn," Kidbrooke Park Road, Blackheath, on 1898, Nov. 26, after an illness of about three months' duration, and was buried on Dec. 1 in Charlton Cemetery.

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* Memoirs R. A. S. vol. xxxii.

Besides the contributions to scientific literature mentioned above, Mr. Dunkin re-arranged a new edition of Dr. Lardner's text-book on Astronomy; he was sole author of a well-known work 'The Midnight Sky,' and contributed to the 'Leisure Hour' and other publications. He was a member of the Royal Institution of Cornwall, and sometimes wrote in its publications.

He married, on 1848, April 4, Maria, eldest daughter of the late Samuel Joseph Hadlow, formerly a member of the Stock Exchange. His widow survives him, also an only child, Edwin Hadlow Wise Dunkin, an antiquarian of reputation, author of 'The Monumental Brasses of Cornwall,' 'The Church Bells of Cornwall,' besides papers in the 'Sussex Archæological Collections.'

H. P. H.

Notes on the Wolf-Rayet Stars.

THIRTY-TWO of these curious objects were examined, spectroscopically and spectrographically, by Professor Campbell with the great Lick refractor in 1892–3 *; and in sorting these out by the alternative position † of their characteristic azure band, I have lately noticed a correlation of some significance. It is this: when the band in question has its maximum at λ 469, the line most prominent in the lower part of the spectrum is at λ 541; while the shifting of the blue glow upward to λ 465 throws the stress of radiation downward upon the yellow line at λ 581. There is an "extreme and mean" correspondence. The inner rays are strong when the outer rays are weak, and *vice versâ*. So far as extant data enable one to judge, this rule is infallible; merely a slight qualification of it occurs in Campbell's remarkable hydrogen-enveloped star, in which the "citron-line" (λ 569) is substituted for the golden ray below it as the brilliant associate of a brilliant λ 465. More than that, it seems to extend to the nebulæ. The green Wolf-Rayet line (λ 541) is an exceedingly rare component of their spectra; those of only three planetaries (N.G.C. 6572, 7027, and 7662) are known to include it; yet all three show beside it the scarcely less uncommon azure gleam at λ 469. The connexion has, besides, an obvious basis in theory, Both radiations, in fact are—although with different degrees of probability—ascribed to hydrogen. That in the green has an established status as the lowest member of the new "Pickering" series; its blue companion occupies the place calculated by Dr. Rydberg for the "fundamental" of the corresponding principal series. There *ought* to be a *sforzato* hydrogen vibration (so to speak) of just its period; and the actual vibration is, under favourable circumstances, very peculiarly emphasized. Nevertheless, the identification encounters grave difficulties.

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* 'Astronomy and Astrophysics,' vol. xiii. p. 448.

† Observed by Vogel in 1873, definitively recognized as marking a specific difference by Sir William and Lady Huggins in 1890, and admitted as a principle of classification by Pickering in 1891.

Chief among them is its seemingly multiple nature. The azure and the lazulite bands—thus to distinguish them—are not really independent. One, usually, if not always (as Sir William and Lady Huggins did not fail to perceive), *echoes* the resonance of the other. Indeed, Professor Campbell recognized several additional bands higher up in the blue as individually separate, yet shown to be of the same family by their fusion with them, in a couple of the observed spectra, into a single beam, fully one hundred tenth-metres in breadth. Hence, as things stand at present, we are not entitled to consider the azure band apart from its fellows; but for them Dr. Rydberg's formula makes no provision.

One of the most singular features * of the Wolf-Rayet stars is the occurrence in their spectra of mixed radiations. That of γ Argûs, above all, is a tissue of them. Professor Campbell observed in it C and D_3 bright, the remaining hydrogen and helium-lines being dark †; and Mr. McClean's photographs showed the new hydrogen-set to be similarly affected. The phenomenon, which recurs in several analogous objects, is so far methodical that the glowing rays are always the least refrangible of each sequence. This is evidently a settled law, belonging to the innate order of the arrangements—whatever they may be—by which these surprising appearances are brought about. A curious example of detailed conformity to it is given by the various helium-series, bright and dark, in P Cygni.

The Wolf-Rayet stars are sharply cut off from nebulæ by the absence from their spectra of any trace of nebulum-emanations. On the stellar side, their affinities are—as Miss Maury has pointed out ‡—with the "Orion" type. Her first group especially, which embraces S Monocerotis, ι Orionis, and the trapezium-quartette, reverses many of their characteristic rays, notably some members of the new hydrogen-series. The missing link was supplied by Professor Pickering's discovery of the compound spectrum of ζ Puppis, exhibiting a dark-line scroll of "early Orion" pattern emblazoned with the *irreducible minimum* of Wolf-Rayet bright beams. For as such, undoubtedly, the azure and lazulite effluences must be regarded. They survive the extinction, or reversal, of all the emissions usually associated with them, as well as their displacement by metallic absorption. Mr. McClean secured at the Cape some large-scale spectrographic impressions of ζ Puppis *, confirmatory, on the whole, of the Arequipa results; but the lower range of its light-gamut remains a *terra incognita.* It is not unlikely to include a vivid C, the other hydrogen-lines being conditioned very much as in γ Argûs; and it will be of interest to know whether the "lettuce-line" (λ 541) of the Pickering-series is bright or dark. A glance through a prismatic eyepiece would suffice to decide, since the star is of 2·5 magnitude. It is not a solitary example—a mere *lusus naturæ.* Half-a-dozen minor objects can be more or less probably inferred to share its transitional nature. They are designated as follows:—

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* It is shared by Alcyone, the leader of the Pleiades.

† F is, perhaps, doubly reversed; it is certainly a complex radiation.

‡ 'Harvard Annals,' vol. xxviii. pp. 15, 47.

Name.	Magnitude.	Remarks.
DM. +44° 3649	7·9	See Astrophys. Journ. iv. 369.
DM. +44° 3679	6·8	Ditto.
Cord. Gen. Cat. 22748 ..	5·9	Campbell's No. 31.
Cord. Gen. Cat. 23072 ..	6·5	Campbell's No. 36.
Cord. Gen. Cat. 23073 ..	7·1	Campbell's No. 37.
DM. +35° 3953	7·0	Campbell's No. 44.

The particular attention of spectroscopists may be claimed for them, their dubious status suggesting the possible presence of critical traits.

No chemical meaning has been assigned, even conjecturally, to the yellow or the citron Wolf-Rayet lines; but the former flashes out near the maximum phases of that unique variable, R Geminorum †; and the latter is among the bright rays recently detected by Professor Hale in fourth-type spectra ‡. The peculiar spectrum of the variable, which may be described as of the fourth type inverted, accentuates the double connexion. Alone among our sidereal acquaintances, the star, like a comet, emits glowing carbon-bands. Vogel's observations of them, it is true, were avowedly insecure, and are now antique, since they date back to 1874. Nor can they be immediately renewed. For the star, at its epochs of brilliancy, is at present lost in the solar radiance, and their periodicity in 370 days prolongs the conjunction through many years. They escape from obliteration with exasperating slowness. When—in the twentieth century—the spectrum characterizing them comes to be photographed, it will be found, one may venture to predict, to include the lazulite blue band at λ 465, heretofore unfailingly associated with the golden ray at λ 581. By that time, too, the association may have been explained—the effect may no longer be without a known cause.

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* 'Spectra of Southern Stars.' plate vi.

† Astr. Nach. No. 2000; 'System of the Stars,' pp. 74, 89.

‡ Astrophys. Journ., Nov. 1898, p. 239.

AGNES M. CLERKE.

Seismological Observations at the Liverpool Observatory *.

IN August 1897 the Seismological Committee of the British Association entrusted one of their seismometers to my care. It was mounted by Mr. Horace Darwin in a cellar of the Observatory, and the photographic record of the motion of a spot reflected from the mirror has been maintained since, save for a few interruptions arising from the failure of the clock or some temporary disturbance. The instrument itself has been described by Dr. Davison in 'Nature,' l. 246, and the general arrangement of the apparatus there detailed, the method of determining the scale, and the directions for the use of the instrument have been followed without alteration at the Liverpool Observatory. The instrument is arranged to measure tilts or displacements in the plane of the prime vertical; the mirror being in this plane, that of the suspending wires being at right angles. Disturbances are shown by the motion of a spot of light being carried to the east or west of its normal position.

The more noticeable deviations from uniformity have been reported to Professor J. Milne as produced probably by earthquake shocks; the dates of these interruptions appear in another part of the report. The instrument, however, does not seem very well adapted for the measurement and discussion of these irregular motions . . . . . The following discussion is therefore confined entirely to the uniform behaviour of the spot of light.

For a very considerable time after the instrument was mounted the photographic trace showed such a continual and rapid motion towards the east that all other effects were completely masked.

[Mr. Plummer here proceeds to describe in detail the difficulties he has met with in the adjustment of the instrument and the apparent discordance of some results.—EDS.]

. . . . I can offer no explanation of these anomalies. I merely mention them here to show that the amount of material at my disposal is not so great as I could wish: that a much longer time is necessary to remove the instrumental and systematic errors, and that the present result is a preliminary inquiry now offered to show what use has been made of the instrument entrusted to my care.

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* [The observations of diurnal variation of level mentioned by Mr. Plummer at the last R.A.S. meeting seemed to be so interesting, as they appear to be similar to certain variations of nadir found elsewhere, that we gladly avail ourselves of his kind permission to publish extracts from his Report to the British Association on the subject.—EDS.]

However distrustful one may be of the result, and however cautiously one may feel it necessary to speak of the numerical values obtained, it is impossible to doubt the general character of the motion of the mirror, notwithstanding the small angles with which we have to deal. A mere glance at the record for any one day is sufficient to show the general features of the curve; and herein I believe the motion in the prime vertical agrees with that derived from meridional displacement, though to what extent the motion precedes or follows that on the meridian I have no data to offer. On all days on which the photographic trace has offered no suspicion of unsteadiness in the scale value, or where no interruption of the trace has been made for more than twenty-four hours, the ordinates of the curve have been measured from the time traced for each hour. These ordinates have been read off to the tenth of a millimetre, corresponding usually to about 0·004 of a second of arc, consequently the third place of decimals has been retained, but simply as a matter of calculation. These measures have been grouped in monthly periods, for it was soon apparent that the time of maximum displacement was not constant throughout the year. The mean values of the ordinates for each month have been compared with Bessel's Interpolation Equation for expressing in the usual periodic formulæ the reading at any hour of the day reckoned from noon, in terms of the mean value and the hour of the day. The principal maximum values, both positive and negative, for each month as derived from the forementioned equation are as follows:—

Month.	Time of Maximum Displacement.	Amplitude.	Month.	Time of Maximum Displacement.	Amplitude.
	h m	″		h m	″
January......	6 17 P.M.	+0·0603	May	4 27 P.M.	+0·0781
,, ...	0 34 A.M.	− ·0602	,, ...	10 55 ,,	− ·0635
,, ...	5 24 ,,	+ ·0323	,, ...	5 1 A.M.	+ ·0377
,, ...	10 21 ,,	− .0353	,, ...	10 34 ,,	− ·0524
February ...	6 34 P.M.	+ ·0597	June	4 10 P.M.	+ ·0516
,, ...	1 26 A.M.	− ·0645	,, ...	0 3 A.M.	− ·1006
,, ...	6 15 ,,	+ ·0319	,, ...	5 48 ,,	+ ·0735
,, ...	11 4 ,,	− ·0497	,, ...	10 30 ,,	− ·0279
March	5 48 P.M.	+ ·0841	July	3 20 P.M.	+ ·0690
,, ...	11 32 ,,	− ·0732	,, ...	9 36 ,,	− ·1038
,, ...	5 27 A.M.	+ ·0221	,, ...	4 38 A.M.	+ ·0351
,, ...	11 5 ,,	− ·0337	,, ...	10 8 ,,	− ·0048
April.........	4 31 P.M.	+ ·0803	August	4 57 P.M.	+ ·0642
,, ...	10 30 ,,	− ·0592	,, ...	11 24 ,,	− ·0727
,, ...	3 43 A.M.	+ ·0210	,, ...	5 57 A.M.	+ ·0738
,, ...	9 36 ,,	− ·0412	,, ...	11 32 ,,	− ·0612

The meaning of the positive sign in this table and elsewhere is that the spot of light has travelled towards the east. So far as the table shows anything, it exhibits a tendency for the first eastern elongation, that in the afternoon, to occur earlier in the day as the year advances, also that the amplitude of the afternoon excursion, whether east or west, is greater than that in the morning, and that the maximum effect occurs in the summer months. It would be wrong to insist too strongly even on these tendencies, considering that only a part of one year has been examined, and certainly premature to suggest any physical interpretation. I hope, however, that this partial result may prove of sufficient interest to induce the Committee to sanction further inquiries of the same nature, for which I think the instrument is peculiarly well fitted. W. E. Plummer.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Cause of the Gegenschein.

Gentlemen,—

I have carefully observed the Gegenschein on many occasions during the past two years, at seasons when it is favourably situated among the stars; but I have never seen any indication of a dark centre, or any such appearance as would tend to confirm Mr. Anderson's theory that it is a contrast effect due to the dark shadow of the Earth projected upon an extension of the zodiacal light.

On the 5th inst., while engaged in photographing the spectrum of Mira, I left the guiding telescope for a few minutes, and on looking up at Cetus my attention was at once arrested by the Gegenschein, situated in the triangular space defined by the stars α Arietis, α Ceti, and the Pleiades. I do not know whether it was due to the exceptional transparency of the air, or to increased sensitiveness of the retina after working in the dark for an hour or so, but on this occasion the light seemed to me more obvious and distinct than I have ever before seen it. I should describe it as a uniformly luminous patch of light, not round, or distinctly elliptical, but much extended in the direction of the ecliptic, being some 20 degrees long by 8 or 10 wide. I estimated the centre to be equidistant from α Arietis and γ Ceti, but slightly to the east of a line joining those stars, say at R.A. $2^h\ 30^m$, Dec. $+14°$; a position which would place it some 3 degrees to the west, or *behind* the anti-sun.

I do not, however, place any great reliance on this estimate, because I found it difficult to judge the R.A. of the centre on account of the elongated shape of the object; but according to Barnard, who has determined the position with great care over a series of years *, there is distinct evidence of a slight lagging of the Gegenschein behind the point on the ecliptic opposite the Sun, and it is this fact which first suggested to me the following theory of the origin of the light.

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* 'Astronomical Journal,' No. 403.

Dr. Stoney has shown that in the atmospheres of planets a molecular sifting-out process must take place; that is to say, the gaseous elements of low molecular weight tend to be eliminated and to disperse into space, because the velocities of such molecules must frequently be great enough to counteract the force of gravity. In the case of the smaller bodies of the solar system, such as the minor planets and comets, and even the Moon, no permanent atmosphere of any kind can exist, as the heaviest molecules of the substances which are gaseous at temperatures to which solar radiation may raise them are moving swiftly enough to counteract the feeble gravitational attraction.

On the Earth, however, gravity is powerful enough to permanently retain the elements oxygen and nitrogen, whilst the swifter-moving molecules of hydrogen and helium can escape, consequently the latter elements are not present in our atmosphere, or only in minute traces. It is, however, highly probable, as Dr. Stoney has pointed out, that these lighter elements are continuously being supplied to the atmosphere from various terrestrial sources; therefore the sifting-out process must be in continual operation, even at the present time.

But what becomes of the escaping molecules? Do they disperse into space equally in all directions?

Now in the case of a comet approaching perihelion evaporation of condensed gases produces a temporary atmosphere which the comet is totally unable to retain owing to its small mass; consequently a quite analogous molecular dispersion occurs. This, however, by no means takes place equally in all directions because the dispersing molecules come under the influence of a force, varying as the surface rather than the mass of the particles, which repels them with enormous velocities from the Sun.

Is it not therefore probable that molecules escaping from the *Earth* are similarly repelled with enormous velocity from the Sun? In other words, why should not the Earth possess a tail of escaping hydrogen and helium similar in every respect to a cometary tail, and visible to us in projection as the Gegenschein!

Such a tail, streaming away for many millions of miles, would not, however, appear exactly opposite the Sun. The orbital motion of the Earth would produce a slight aberration displacing it a little to the west, the amount of the displacement depending on the ratio of velocities of the Earth in its orbit and the matter streaming away. Thus the slight lagging of the Gegenschein behind the anti-sun is accounted for. But the velocity of the escaping molecules would not be constant, under the influence of the force of solar repulsion they would move away with an accelerating velocity, and probably the lighter hydrogen molecules would be repelled with much greater force than the heavier molecules of helium. The tail would therefore be more or less curved, and would be of a composite structure, exactly as in the case of the cometary tails investigated by Herr Bredichin.

If we assume the existence of such a tail of extremely attenuated gaseous matter, I think that, viewed end on, it would present just such appearances as the Gegenschein is known to present.

We should, for instance, expect to find it varying somewhat both in shape and position, while keeping pretty closely about a mean position nearly opposite the Sun. It would, of course, be visible to us by reflected sunlight, notwithstanding that the shadow of the Earth would be projected upon it.

That our hypothetical tail must be of the last degree of tenuity is evident, seeing how small an amount of matter is required to produce even the most splendid tail of a comet. But, it may be asked, why should we alone be favoured with such an appendage? why, for instance, should not Venus or Mars be furnished with tails?

It is, evident, however, that the illumination of our atmosphere near so bright an object as a major planet would completely obliterate any such tails as they might reasonably be supposed to possess. Our own tail is, of course, most favourably situated for observation, being always in opposition! Yet it is but a poor affair after all, and can only be seen with any certainty when in a part of the sky free from planets or bright stars.

The concluding lines of Mr. Anderson's letter in your last number make it unnecessary for me to offer any apology in submitting for his acceptance so outrageous a theory!

Yours faithfully,

Kenley, 1898, Nov. 13. J. Evershed.

Periodical Comets due in 1899.

Gentlemen,—

The comet which was discovered by Mr. Denning on Oct. 4, 1881 (nearly a month after perihelion passage), and computed to have a period of about 8·76 years, escaped observation in 1890, probably owing to its unfavourable position on that occasion. According to Dr. Matthiesen's elements, it should return to perihelion early in the ensuing month of February. Herr Berberich has published (Ast. Nach. No. 3524) a search-ephemeris (referred to in the November 'Observatory'), by which it appears that it will fall to our collaborateurs in the southern hemisphere to look for it, and we shall hope soon to hear of their success.

Tempel's comet (1866 I.), connected with the Leonid meteors, should return some time in the spring, though the conditions of its visibility will not be very good on this occasion.

Thus far of periodical comets due in 1899 which have been seen with certainty at only one previous appearance. We have now to speak of others which have established their claim to periodicity by being seen (with one exception) at more than one approach to perihelion.

The first of these is that discovered by Mr. H. P. Tuttle on the 4th of January, 1858, on which occasion its periodicity was detected, though it had been previously detected by Méchain at Paris in 1790, who was the first to see it on January 9, and afterwards calculated a parabolic orbit. Bruhns, in 1858, determined it to be elliptical, with a period of about 13¾ years; it duly returned in the autumn of 1871, and again in 1885, when it passed its perihelion on the 11th of September, so that another return will be due next summer.

About the same time Tempel's second periodical comet will be due. This was discovered at the Brera Observatory, Milan, on the 3rd of July, 1873; its period is about 5¼ years, and it was observed again in the autumn of 1878, but not seen at the returns due in 1883 and 1889, at which its positions were unfavourable for observation. It was, however, redetected in May 1894, after the perihelion passage on April 24, and another return may be expected about next midsummer.

The comet discovered by Mr. Holmes at Islington on the 6th of November, 1892, was calculated to have a period of about 6⅞ years. This is remarkable for having an orbit more nearly circular than that of any other comet. It is difficult to account for its non-detection before the date of Mr. Holmes's discovery, except on the supposition that it had then recently undergone a considerable increase in brightness. A return will be due next summer.

Finlay's comet (discovered on the 26th of September, 1886) will be also expected to return next year, but not until the autumn or winter. Its period is about 6½ years in length, and on the last occasion it was first seen on May 17, 1893, passing its perihelion a month afterwards (on June 16).

Yours faithfully,

Blackheath, 1898, Dec. 5. W. T. LYNN.

Ten Comets in one Year.

GENTLEMEN,—

The observation of ten comets in 1898 furnishes a record in this branch of astronomy. Three of the comets seen were returns of known periodical comets, and seven were new. The years 1857, 1873, 1881, 1889, and 1892 supplied seven comets, while 1858 furnished eight; but the present year has exceeded these figures, and it is remarkable that two of the comets of 1898 were accidentally discovered by means of photography. The following is a general summary: I have not given the elements, as in the majority of cases they have not been definitively ascertained:—

1898 I. (*b*).—Discovered by Perrine on March 19. Perihelion passage March 17. Elliptic elements have been computed by Perrine giving a period of 305·2 years. Pokrowskij finds 322·6 years.

1898 II. (*a*).—A return of Pons-Winnecke's comet. Detected by Perrine on January 1. P.P. March 20.

1898 III. (*c*).—A return of Encke's comet. Detected by Grigg at New Zealand on June 7, and by Tebbutt at Windsor, N.S.W., on June 11. P.P. May 26.

1898 IV. (*f*).—A return of Wolf's Comet. Detected by Hussey on June 16. P.P. July 4.

1898 V. (*g*).—Discovered by Giacobini at Nice on June 19. P.P. July 25.

1898 VI. (*e*).—Discovered by Perrine on June 14. P.P. August 16. The elements are very similar to Pons's Comet of 1812 and 1884 I.

1898 VII. (*d*).—Discovered by Coddington on June 11 on a photographic plate, in the immediate region of Antares. Independently found by Pauly at Bucharest on June 14. P.P. September 14.

1898 VIII. (*j*).—Discovered by Chase by photography on November 14 in the position of the radiant-point of the Leonids. P.P. September 19.

1898 IX. (*h*).—Discovered by Perrine on September 13, and independently by Chofardet on the following day. P.P. October 20.

1898 X. (*i*).—Discovered by Brooks on October 20. P.P. November 23. The comet was pretty bright, and passed from Draco through Hercules to the south.

Yours faithfully,

Bristol, 1898, Dec. 20. W. F. DENNING.

The Name of Planet DQ.

GENTLEMEN,—

In your last number Mr. Chandler suggests the name Pluto for this planet. It does seem desirable that a permanent designation should be given to a body so unique amongst the small planets and likely to be so useful in astronomical inquiries. But surely not that of

> "Pluto, the grizzly god, who never spares,
> Who knows no mercy and who hears no prayers."

I cannot help agreeing with "A Fellow of the Royal Astronomical Society," whose designation, not name, appears so often in another place, that this would not be at all appropriate. Besides, it has ceased to be a rule to give a small planet the name of a deity, although a feminine form is retained. This leads me to a suggestion which may, perhaps, be considered acceptable. The provisional designation DQ has become very familiar to our ears. Why should it not be formed into a word, slightly altered, as Dequa? This may be tacitly understood as the beginning of a sentence, "De quâ planetâ potest solis distantiam veram investigare."

Yours faithfully,

Blackheath, 1898, Dec. 21. W. T. LYNN.

Sunset from a Legal Point of View.

GENTLEMEN,—

What provision does the law make for a cyclist who has been arrested for riding without a lamp while the sun is visible above the horizon? On one interpretation of the law this might happen in the west of Rossshire in the month of June.

Yours very truly,

Sigglesthorne, Hull, 1898, Dec. 9. D. R. FOTHERINGHAM.

OBSERVATORIES.

THE CITY OBSERVATORY, EDINBURGH.—The Observatory on Calton Hill has had a somewhat chequered history. The foundation stone was laid in 1776, the idea being that a building should be erected to house some instruments which had belonged to James Short, the optician. A lease was granted to the brother of James Short on condition that the public should be admitted for a small fee. Mr. Short and his family were to receive these emoluments, but the buildings and instruments were vested in the Town Council, and were ultimately to become public property. Adam, the famous architect, seems to have spoiled a good idea by architectural suggestions which quite unfitted the building for an observatory, and the plan of a public observatory never came to fruition. About 1811 some scientific men took the matter in hand, and the Town Council allowed the use of the land and the observatory on condition that if this should cease to be used as an observatory all should revert to the town. New buildings more fitted for an observatory were erected adjacent to those standing, and when these were nearly completed, in August 1822, George IV. visited the city and granted permission to call the institution a Royal Observatory.

It is well known that, in 1889, it was decided to remove this Royal Observatory to a new site on Blackford Hill, so that then the establishment on Calton Hill reverted to the Town Council, who decided to carry out the idea of a century ago and equip an observatory for the benefit of the citizens. Mr. W. Peck was chosen as Director, several persons presented instruments, and on October 24 last the new institution was formally opened by the Lord Provost. The instruments at present installed are a silver-on-glass reflector of 13 inches aperture and 11 feet focus, on an English equatorial mounting, presented by Mr. Cox; a reflector of 13 inches diameter, presented by Lord Crawford; a refractor of 6 inches aperture and 9 feet focal length, the gift of Mr. McEwan, mounted equatorially with electric-clock control—this instrument can be used for photography, and has as an accessory a direct-vision spectroscope of 12 prisms. Mr. McEwan also presented a barrel chronograph, for use with the transit instrument, of 6½ inches aperture and 8 ft. focal length. There is also a small transit instrument for the use of students, and in the dome to the north-east of the main building there is a large refractor of 22 inches aperture and 30 feet focal length, on an equatorial mounting of the German form.

As to the work to be done with this large installation, the Lord Provost, in his speech, remarked that "Arrangements would be made whereby the public would be admitted on certain evenings, by ticket, and rigidly in order of application, as the number of visitors they could accommodate at one time was necessarily limited. Something, too, would be done in connection with the senior pupils of the higher-grade schools, as well as those directly interested in the practical application of astronomy, as in navigation, surveying, and civil engineering. In the direction of time-keeping the Observatory would also be employed, as it was proposed to have a system of clocks distributed over the city, and controlled by the standard mean time clock of the Observatory."

Madras.—The Report for the year 1897–98 consists of two parts. In the first six pages Prof. Michie Smith sets out the work of the Madras Observatory, and shows the progress made with the observatory at Kodaikánal. Prof. Michie Smith has a staff of seven native assistants, who have been employed in making meteorological observations, keeping the Time Service going, in preparing the Catalogue for press, and in deducing new instrumental errors for the purpose of re-determining the latitude from some existing 2800 observations. The value agrees well with that of the Trigonometrical Survey:

$$\text{Lat. } 13^\circ\ 4'\ 7''{\cdot}98 \pm 0''{\cdot}045 \text{ N.}$$

The Astronomer Royal visited the Kodaikánal Observatory in February and discussed the plans of the new buildings, with the result that of the three domes only two are to be completed at present, but are to be 18 feet diameter instead of 15 feet, the place of the third being left in the hope that a large telescope may in the future be placed there. As regards the eclipse, it will be remembered that a well-equipped party set out from Madras and camped at Sahdol. The lens of the Kodaikánal photospectrograph was brought into use, and formed what is now known as the 40-feet instrument (this lens is 6 inches clear aperture and 40 feet focus). This was the principal instrument; it gave seven photographs of the coronæ, which Prof. Michie Smith believes are good. The diameter of the Moon on these pictures is 4¾ inches. The photographs taken with the photoheliograph give a diameter of 0·6 inch, and the corona can be traced for two diameters. At present the points to which he would call attention are:—

(1) The extreme brightness of the inner corona and the rapidity with which it fades away.
(2) The evident connection between prominences and certain details in the corona.
(3) The striking resemblance between the coronas of 1896 and 1898.

Melbourne.—We always read the Report from the Government Observatory of Victoria with interest. For the past few years it has presented a graphic picture of a good man struggling with adversity; but in Mr. Baracchi's report for the twelve months ending 1898, June 30, there are some signs that financial difficulties are being overcome and that arrangements are being made whereby we may shortly expect to see the various branches of work of the institution in full swing. The Board of Visitors, in their report to the Governor of the Colony, state that the work has been carried out assiduously and systematically, and that the best use has been made of the instruments, and they call attention to Mr. Baracchi's anomalous position. Since Mr. Ellery's retirement he has had full control of the observatory, but has only been styled Acting Astronomer, and has received no increase of emolument above that which he previously enjoyed. They hope that this will soon be remedied.

The work accomplished with the transit-circle during the period consisted of 2060 observations of right ascension and 1202 of north polar distance. Beyond the observations for time adjustments the stars observed were those to be used as standard for the formation of the Astrographic Catalogue.

As to the Astrographic work, 37 plates were added to the catalogue series and 132 to the chart, which completes the total of the catalogue plates required, and brings the number of chart plates taken to a total of 278 out of the 1149 required. Arrangement has been made with the Government Astronomer of New South Wales that the catalogue plates taken at Sydney and Melbourne observatories shall all be measured at the Melbourne Observatory, the two colonies sharing the expense of the undertaking; and it has been settled that four young assistants (preferably women) should be employed for this work, assisted by one of the officers of the Melbourne Observatory, at an aggregate cost of £257 a year, which is little enough. Mr. Baracchi takes a great personal interest in the subject of Terrestrial Magnetism, and besides the photographic registration of the variations of magnetic elements and the determinations of absolute values, which have been continued as heretofore, he will shortly take in hand the reductions of these observations made during the past 30 years, for which a small sum of money has been placed on the estimates. A set of instruments has been borrowed from the Kew Observatory, which will probably be used in a Magnetic Survey of New Zealand. A great movement is on foot to initiate such a survey, and to establish a permanent observatory in the island. Mr. Baracchi recommends the purchase of a Ewing's horizontal pendulum, as the seismograph at present in use is not sufficiently sensitive.

The post of Chief Assistant is still vacant. Mr. Ingamells, who left the observatory a few years ago, has again joined the staff. It is worthy of note that for the post which he occupies, and which carries a salary of £150, there were 45 applications, several candidates possessing high university degrees. Mr. Ingamells was chosen on account of his experience.

NOTES.

Comet Notes.—Herr Bauschinger, of the Berlin Rechen-Institutts, has published an elaborate investigation of Brooks's Periodic Comet (1889 V.–1896 VI.), using all the observations of both apparitions and connecting the two by a calculation of the perturbations in the interval. The latter had been already done by Mr. Chas. Lane Poor, and the two results agree very closely. The following are the final elements for 1896:—

Epoch	1896 Oct. 11·5 Berlin M.T.	
M........	356° 42′ 36″·9	
ω	343 47 57·7	1900·0
☊	18 4 18·3	
i	6 3 34·3	
φ	27 59 59·6	
μ	499″·9736	

The observations are well represented. The author suggests that some small systematic errors may be due to the centre of gravity not coinciding with the centre of brightness. The comet had several faint companions in 1889, but these were not seen in 1896.

Herr J. Müller gives in Ast. Nach. 3531 the following elements of Chase's Comet (*j* 1898) from observations on Nov. 14, 23, 25, Dec. 5:—

T	1898 Sept. 19·649 Berlin M.T.
ω	4° 23′ 9″
☊......	95 47 0
i	22 28 25
log *q* ..	0·35792

Identity with the Comet 1867 I. has been suggested, but does not seem to be possible.

Ephemeris for Berlin midnight.

	R.A.			N. Dec.			R.A.			N. Dec.	
	h	m	s	°	′		h	m	s	°	′
Jan. 1....	11	3	40	28	40	Feb. 2....	11	8	23	34	40
5....	11	5	50	29	23	6....	11	7	8	35	21
9....	11	7	33	30	9	10 ...	11	5	35	35	58
13....	11	8	48	30	55	14....	11	3	46	36	31
17....	11	9	35	31	41	18....	11	1	45	37	2
21....	11	9	55	32	28	22....	10	59	38	37	27
25....	11	9	48	33	13	26....	10	57	29	37	47
29....	11	9	16	33	58	Mar. 2....	10	55	19	38	3

The brightness during January is about equal to that at discovery, after which it slowly declines; the comet is a faint object and difficult to observe.

Coddington's Comet (*c* 1898) has been observed at Mr. Tebbutt's Observatory, Windsor, N. S. Wales, on 67 nights, extending from June 15 to October 18. The observations comprise 545 micrometer comparisons and 92 comparison stars. After Oct. 18 the Moon and cloudy weather interfered with the work, but Mr. Tebbutt hopes during the absence of the Moon in the beginning of November to pick up the comet again.

Mr. Tebbutt, who sends the above information, points out a misprint in his letter on p. 315 of the August number. The declination in line 15 of his letter should be 11° 34′ instead of 10° 34′ as printed. A. C. D. C.

Minor Planet Notes.—M. Fayet gives the following orbit of 433 (DQ) in Ast. Nach. 3530; it is derived from seven observations about Aug. 16, six about Sept. 17, and two on Oct. 22; it satisfies the December observations better than Millosevich's orbit given last month, the correction required by Fayet's ephemeris on Dec. 8 being R.A. $+0^s.21$, dec. $+9''.8$:—

T	1898 Aug. 16·5 Paris M.T.		
M	213°	1′	42″·4
π	121	16	27·3
☊	303	30	19·8
i	10	49	36·7
ϕ	12	51	53·9
log a	0·1636612		
μ	2016″·11		

M. Fayet proposes to compute definitive elements of this planet, and requests that any unpublished observations may be sent to him.

The following ephemeris for Berlin midnight is by Millosevich (Ast. Nach. 3532):—

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
Jan. 1....	22 53 27	3 47	Jan. 17....	23 32 56	7 18
5....	23 3 0	4 38	21....	23 43 21	8 13
9....	23 12 45	5 30	25....	23 53 59	9 10
13....	23 22 44	6 23	29....	0 4 51	10 7

The brightness during January remains stationary at 12·5, the approach of the planet to the Sun balancing its recession from the Earth.

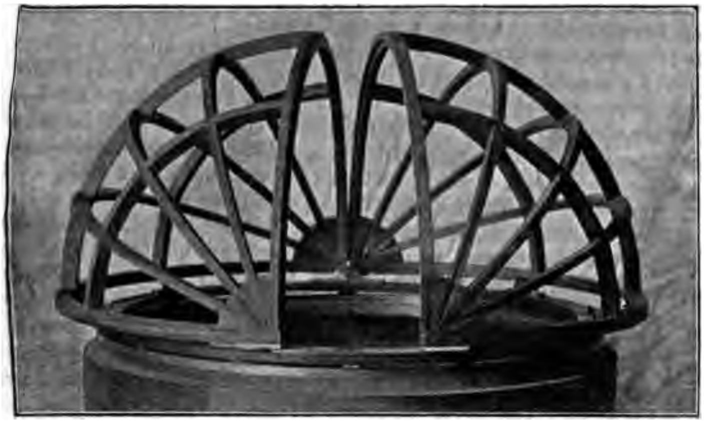
Prof. Newcomb gives the following ephemeris of 433 for the next opposition (Ast. Journ. 450). This is based on elements by Prof. Hussey, and may be considerably in error, but it will give some idea of the circumstances of the next return:—

1900.	R.A. h m	N. Dec. °	Δ.
Nov. 15......	1 56	54·7	0·358
20......	1 45	54·0	0·346
25......	1 38	52·7	0·336
30......	1 32	51·5	0·328
Dec. 5......	1 30	49·7	0·322
10......	1 32	48·2	0·314
15......	1 34	46·0	0·309
20......	1 43	43·9	0·308
25......	1 48	41·3	0·308

As this will be the most favourable opposition for many years, advantage should be taken of it for improving our knowledge of the solar parallax. It is hoped that as many observatories as possible will co-operate in the work, both by heliometer observations and photography. The planet will not be well placed for observatories in the Southern Hemisphere on account of its high north declination.

Three more planets have been discovered, viz. EB, EC by Mr. Coddington at the Lick Observatory on Oct. 13, and ED by M. Charlois at Nice on Dec. 8. A. C. D. C.

A NEW DOME FOR EQUATORIALS.—An important part of an observatory, and of which the capable performance adds not a little to the comfort of the astronomer beneath it, is the dome. We give here a picture from a photograph of the model of the latest design in domes by Messrs. T. Cooke & Sons, of York. It will be seen that there is no shutter, a plan which had its beginning, we believe, in the dome of the Transit Pavilion at Greenwich, erected in 1891 from designs by the Astronomer Royal. The dome in that case consists of two halves, which roll east and west, leaving an opening in the meridian, and speaking



with some experience of transit-rooms, we can say this is the most convenient roof for a transit instrument we have yet met with. The idea was followed up in the dome for the new altazimuth at Greenwich, where the dome again consists of two halves, mounted on a ring-girder which revolves so as to put the opening in any azimuth. Messrs. Cooke's plan appears to be the next step in the evolution of this idea: we add their description:—

> Our object was to do away with the usual shutter, covering an opening in the dome. To carry this into effect the dome is mounted on a horizontal revolving ring, or annular frame, provided with rollers at suitable intervals apart, which roll on a fixed circular track or rail firmly secured to the wall of the building. The dome is made in two halves, divided in a vertical plane through the centre, and hinged at one extremity of this division at two of their adjacent corners to a pin fixed to the revolving ring or annular frame above mentioned. At the opposite corners of the two halves of the dome are fixed rollers, one at each corner, and two other rollers are supplied 90° from the first pair, one in each half. These rollers roll on the surface of the revolving ring, or on short races, in such manner as to support the two halves of the dome on the horizontal revolving ring and permit of their being opened and closed. The opening is of course a wedge-shaped one. The two halves of the dome being mounted on the ring, which is capable of revolving by means of the rollers resting on the circular rail, the wedge-shaped opening in the dome can be brought to face any part of the sky.
>
> It may be mentioned as a further advantage of this arrangement that in the winter season, when the dome may be covered with snow, that it does not prevent its being opened, as is so frequently the case with domes with shutters; and furthermore it gives facility for any design of dome being used, which a shutter renders impossible.

OBSERVING AT THE MANORA OBSERVATORY.—Doubt has been sometimes hinted as to the actuality of the objects which Herr Leo Brenner claims to have observed with the 7-inch telescope of the Manora Observatory. As example of this, we recall observations of the Companion of Sirius ('Observatory,' No. 254, p. 256), which some persons said could not be seen with any telescope of less aperture than 20 inches. Herr Brenner writes to us that he now has unprejudiced and impartial corroboration of his statements. Prof. Dr. O. Knopf, of the Jena Observatory, paid him a visit, and was much struck with the observing possibilities at Manora. He saw the Companion to Sirius (presumably the Clark Companion) with powers of 310, 410, and 196, and markings on Mars with a power of 830 *after sunrise*, after which he finally declared, "Now I see that you really observe under exceptional conditions such as no other observatory enjoys" (Prof. Knopf has had experience of many observatories).

We give this with no intention to express an opinion in the controversy of large *versus* small telescopes, but merely in justice to Herr Brenner. The testimony of an experienced man who *has seen* is worth more than that of many who *think they could not.*

THE TIME-SYSTEM OF SOUTH AUSTRALIA.—It seems that though the advantages of the zone time-system are sufficient to justify its adoption over almost the whole world, there are sometimes local reasons which cause it to be viewed with disfavour. This is the case in Adelaide, South Australia. At the Postal and Telegraph Conference held in Australia, in 1893, it was suggested that the time of the 135th meridian, which nearly bisects the continent, should be adopted for all the colonies; but, in accordance with legislation, since February 1895 the time in use in South Australia has been 9 hours fast on Greenwich, and in the adjacent eastern colonies 10 hours fast on Greenwich. This the people of Adelaide hold to be a grievance, because they say that by having hours earlier their business competitors who live in the eastern zone have an undue advantage. Also, since the longitude of Adelaide is about $9^h\ 14^m$ E. there is not as much daylight after office hours as there should be for the young men to play cricket, and for the same reason there is an increase in gas-bills. So rather than make a small change in their office hours, the people of Adelaide propose to change their time-system, and a bill has been drafted which shall make the standard time 9½ hours fast on Greenwich.

Of paradoxes and hard cases based on local time there are no end. The latest we have seen refers to an insurance policy. A resident in one of the western States of America insured himself against accidents for a year, dating from noon on November 13. At eleven o'clock in the morning of November 13 in the following year he met with an accident and claimed. The company resisted on the ground that as their head-office was in New York, the time stated on the policy was noon at that city, and that as the accident happened at 11 o'clock in the zone two hours west, the policy had already lapsed.

The Total Eclipse of the Moon on December 27 ended in disappointment to observers in the neighbourhood of London. A completely overcast sky and a downpour of rain throughout the whole evening rendered any attempt at observation hopeless.

We regret to see the announcement of the death of the Rev. Bartholomew Price, Professor of Natural Philosophy in the University of Oxford. Prof. Price had been a member of the Board of Visitors of the Royal Observatory for many years.

The latest example of the munificence of Miss Catherine Bruce is reported in 'Science.' She has provided the means for purchasing a photographic telescope for Columbia College Observatory, which will be mounted at Helsingfors and used by Dr. Donner to take plates of the pole with star-trails to be measured by Prof. Jacoby. Miss Bruce has also given a sum in aid of the computing work of the Observatory.

The Paris Academy of Sciences has awarded the Lalande Prize to Dr. S. C. Chandler, and the Damoiseau Prize to Dr. G. W. Hill.

The death is announced of Prof. H. W. Vogel, of the Berlin Technical High School. Prof. Vogel was an early worker in the field of spectroscopic photography, and was one of the first to photograph by means of the rays at the red end of the spectrum. It may be well to mention that this gentleman is not the Director of the Astrophysical Observatory at Potsdam.

A new illustrated astronomical periodical will shortly be published from the Manora Observatory, Lussinpiccolo, under the title 'Astronomische Rundschau.' The subscription will be 12 marks for a volume of 400 pages, post-free.

From an Oxford Note-Book.

The Leonids were not a complete failure after all. They were seen at the Cape and in America, and they suggested a brilliant illustration in Sir E. Monson's speech in Paris. Reading it through carefully I am rather afraid that our Ambassador regards meteors either as "beams emitted" by "members of the stellar system" or as "the natural product of an atmosphere highly and dangerously charged with electricity." But here is the passage for others to criticize or admire as they wish; it seems to me worth preserving for reference:—

Political Shooting Stars.

> There are towards the close of every year two terrestrial phenomena, one celestial, the other terrestrial, to the occurrence of which we have accustomed ourselves, and the absence of which we should consequently consider portentous. The two phenomena in question are the appearance of shooting stars in the firmament and of political stars on provincial platforms. But it would seem as if an equation had in some mysterious manner been established between these two phenomena, and that the failure of the meteoric display in the heavens, which should have come off in November, has been redressed by the large increase in the corruscation of eloquence which has been flashed upon appreciative audiences, urban and bucolic, throughout the provinces of our native land. Not only the greater and more luminous components of the constellation which rules the destinies of England, but also the minor members of the stellar system, have been emitting beams of more than ordinary brilliancy, and dazzling observers by their unwonted radiance. This outburst has naturally not been without its effect abroad, and especially in this country has provoked much criticism and comment. The denunciation has been considered in France to have been rather a display of fireworks, artificially designed and exploded, than the natural product of an atmosphere highly and dangerously charged with electricity, the result of friction systematically and injudiciously applied.

Alongside this extract I may as well put another from the pen of a great actor. The meaning assigned to the well-known term "nebular hypothesis" is not that in regular use, but is quite as fair a rendering; and if the term is wanted in this new sense, it would seem rather selfish to insist on rights of mere priority:—

> Like all great works, Hamlet is distinguished by simplicity; he who will approach this subject with the mind of a child will see clearly—it is only when we look at Hamlet as through the blurred microscope of super-subtlety that it becomes a nebular hypothesis.

BEFORE leaving the Leonids I may record an amusing printer's error. Prof. Barnard wrote in his account of the observations, "We have a few *counts* here that may settle the maximum." This appeared in proof "We have a few *comets* here, &c." This seemed too startling a statement to be true, and was accordingly corrected, although it has been a record year for comets. Indeed if the number discovered increases at this rate we may have a comet DQ (or *dq*) as well as a planet; for I see that the 'Astronomische Nachrichten' is to return to the old method of designating comets by letters in the order of discovery instead of by Roman numerals in the order of perihelion passage. Might one suggest that it would be better still to use *both* methods? There is no reason why a comet should not be called Comet *c* 1899 IV. (Smith), Smith being the discoverer; and it would be in many ways useful to have the three items of information always given. And as comets have been mentioned, I would ask, quite irrelevantly, whether the sailor's description of a comet is well known—"a star that has sprung a leak."

DR. CHANDLER'S suggestion of the name Pluto for Minor Planet 433 (DQ) seems a good one, and in default of any better I hope it will be adopted. It is some time since we could feel much interest in the naming of a new planet, and for old association's sake we might almost hope for one or two rival suggestions. There is no real hurry about the name, for "DQ" is in itself a fascinating interim designation. The last story I heard about naming a minor planet was told by the late Dr. B. A. Gould. As I remember it, he had already named Hekate (100) and Helena (101), and was honoured by being asked, at a meeting of the Astronomische Gesellschaft to name No. 301. He announced enigmatically that not only had he the name of the planet in his head, but he had been in *her* head, naming *Bavaria*; for there is a monster statue of Bavaria into the head of which visitors can ascend, and Dr. Gould had dutifully made the ascent.

BEST congratulations to Dr. Chandler on his Lalande Medal. We are glad to think that England was the first to honour his great discovery, but we can also find much satisfaction in this confirmation by our neighbour across the Channel. There is, I believe, a Russian proverb to the effect that "The Czar may succeed in shaking the Earth, but he cannot make it deviate from its axis."* Perhaps Russia, knowing this limitation of the Czar's powers, will follow the lead of France in appreciating the discovery of the way in which the Earth's axis does deviate from its normal position.

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* See 'Illustrated London News,' 1896, May 23, p. 666.

In the recently published Annuaire for 1899 of the Bureau des Longitudes there is an account of the huge telescope, of 49 inches aperture and 200 feet focal length, which is being built by M. Gautier for the Paris Exhibition of 1900. The telescope is to be placed horizontal, and pointed to a Foucault siderostat, with mirror 6½ feet in diameter. I do not gather whether the telescope is to be accurately horizontal; if so, the full aperture will not be available for the Pole; but by giving an inclination of 10° or so to the telescope any portion of the northern sky could be reached at some time. The return to telescopes of great focal length is an experiment of which the event will be awaited with great interest by astronomers.

It is to be hoped that the expectations of the public from the big telescope will not be raised too high by the daily press. The 'Daily Mail' of November 2 gives a fancy picture of the general arrangements, with a single mountain in the Moon thrown on a screen, the height of the mountain being about 5 or 6 feet. Under the picture is written "to bring the Moon within two miles," representing, I suppose, a magnifying-power of about 120,000. It can only result in disappointment and disgust with astronomy if expectations of this kind are raised, which cannot be realized, though I fear some exaggeration is inevitable. Any Americans who are coming to Paris ought to be warned that they can see the Moon much more comfortably by looking at Schmidt's twenty-foot model of it, which is apparently in Chicago, at the Field Columbian Museum. It was made in Germany between 1849 and 1854, in 116 sections; and was purchased by G. C. Riverston, who hoped to make money by exhibiting this and other curiosities in the United States. But when he had brought his exhibits there he was charged so much for duty, and his audiences were so small, that he lost his fortune. The model of the Moon fell into the hands of Lewis Reese, who for ten years has had the big boxes containing it piled up in his workshop, and has now given it to the Field Museum.

After the Total Solar Eclipse of 1896 it was gravely telegraphed to Tokio, "The eclipse has occurred." According to a London paper of this morning (Dec. 28), they do not know whether the lunar eclipse of last night occurred or not. We in Oxford can vouch for the eclipse, though it took place behind so much haze as to be of little use. But the eclipse will, no doubt, live in history as having been seen by the German Emperor, who specially visited the Potsdam Observatory for the purpose.

THE OBSERVATORY,

Corrected times of Rising and Setting of Neptune.

To be inserted in the 'Companion,' 1899, p. 8.

2	38	10	44	18	50
0	37	8	43	16	49
22	38	6	44	14	50
20	41	4	47	12	53
18	45	2	52	10	59
16	52	0	59	9	6
14	53	23	1	7	9
13	0	21	8	5	16
11	5	19	13	3	21
9	8	17	16	1	24
7	9	15	17	23	25
5	8	13	16	21	24
3	8	11	15	19	22

[illegible] Astronomy.'

A vote of thanks was accorded to the donors.

The President. I will ask Mr. Wesley to show the three plates we have received from Dr. Wolf, which illustrate the discovery of minor planets by photography.

Mr. Wesley exhibited lantern-slides made from these plates. The first, taken in 1892 March with a portrait-lens of 6-inch aperture and 80 cm. focal length, showed a trail due to the motion of a minor planet during the two hours' exposure. The second, taken in 1896 January with the same instrument with an exposure of 3 hours, showed two trails, and the third photograph was of the region about γ Aquilæ, with a long exposure of $10^h\ 35^m$. It was remarked on this last plate how clearly the bright stars were defined and stood out relatively to the mass of faint stars. The ratio of focal length to aperture in the case of this lens is much the same as in the case of the Willard lens of the Lick Observatory.

Prof. Turner. My first paper refers to the new instrument described by Dr. Gill at the last meeting for measuring astrophotographic plates. I said at the last meeting that I thought the chief modification introduced by Dr. Gill into the form of instrument used at Greenwich and Oxford, namely, the introduction of two screws to measure the coordinates on the plate instead of the scales in use at Greenwich and Oxford, was not an advantageous modification, because, by his own showing, Dr. Gill is aware that screws are liable to wear away in the course of many micrometer measures. At the time this remark was only made in a general way. I have since had an opportunity of looking more closely into the matter and I find the case is even more against Dr. Gill, in my opinion, than I thought at the time. Dr. Gill presented at the last meeting a paper in which he gave about $0''\cdot3$ as the amount of wear which the micrometer-screws used for his transit-circle had undergone in 10 years. During those 10 years 50,000 observations had been made with them. Now, last year at Oxford we have made 150,000 measures of star-positions; consequently we should expect three times this wear of the screws in one year if we used screws instead of scales. Dr. Gill's were not ordinary screws which might be improved upon, but the improved steel screws which he had adopted after knowledge of wear gained 10 years ago. If there is likely to be wear amounting to nearly a second of arc per year it will be useless to use screws for the work that must be undertaken in measuring the astrographic plates. That is the main point, but there are other minor disadvantages, described in the paper.

The other paper is of a different kind—I admit at once that its character is somewhat speculative. We have known for some years past that Mr. Denning has been urging that there are stationary radiant-points of meteors. We are familiar with the fact that if there were parallel streams of meteors intersecting the Earth's orbit at successive points the motion of the Earth itself would cause an apparent shift of the radiant, much as a star-place shifts owing to aberration. With that fact we are tolerably familiar, and we find the apparent shift of the radiant illustrated in the case of the Perseids and other showers. Mr. Denning was, I think, the first to point out that in other cases the radiant instead of shifting remains apparently constant in position for several weeks and months, and he is inclined to say for the whole year together. He has expressed that view to me in correspondence recently. He is of opinion that meteors may be seen coming from the same radiant as accurately determined as it may be for the whole year round. Now, one explanation of such a phenomenon might be that the velocity of the meteors was infinite compared with the Earth's velocity in its orbit, so that in combining the two to get the relative velocity the Earth's component was negligible, and the radiant remained fixed in position. But this supposition will not accord with direct observations of velocity. In fact there has been found considerable difficulty in explaining on any supposition the fact that a radiant should remain stationary. So difficult has it been found that many experienced observers, such as Col. Tupman, have denied the fact and have said that Mr. Denning's observations were due to the confusing of paths which did not belong to the same radiant. I could not help feeling that Mr. Denning was too good an observer to have made such a mistake. Although I know nothing of meteor observations, accident led me to devote some weeks' thought to the subject, and having hit upon an idea, which I cannot consider wholly satisfactory but which Mr. Denning and Prof. Herschel have been kind enough to say is worth recording, I now submit it. The general idea is that stationary radiants may be due to the scattering of an originally compact swarm round the Earth's orbit by the attraction of the Earth itself. The attraction of the Earth on meteors which are seen in our atmosphere has been ably dealt with by Schiaparelli and others, but, so far as I have been able to ascertain, no one has dealt fully with the effect of the Earth's attraction on meteors which continue to pass by on one or the other side at successive returns, so that the action of the Earth is cumulative. The chief effect of this action I take to be that the meteors are *hurried up* in their passage past the Earth, so that they describe slightly different orbits at their next return. Supposing first of all that the Earth is stationary and that a meteor-swarm is approaching it. Some meteors will meet the Earth and be stopped; others will pass by in hyperbolas of somewhat large eccentricity. The net result of the Earth's action may be summed up thus:—in the first place, the ultimate velocity of the meteors will be of the same *magnitude* before and after the encounter. The velocity will not be the same in *direction.* There will be a deflection of the meteors which will be in the opposite sense according as the meteor passes to the right or the left of the Earth. This deflection, however, may be annulled if we can suppose that the meteor passes alternately to the right or left of the Earth. It can be shown later how a meteor may pass on successive returns to the right and left, so that although at each return there is a definite deflection of the path, on the whole we may imagine the mean deflection to be zero.

There remains an important effect which is cumulative, being always in the same direction at every return; and this is the shortening of the time spent within the sphere of the Earth's attraction. Within this sphere the velocity is everywhere greater than it would have been had the Earth not existed. This is the effect which seems to me to afford a basis for the explanation of stationary radiants.

Let us pass now to the actual case of the Earth in motion, and by the familiar device let us reverse for both Earth and meteors the velocity of the Earth in its orbit, and the attraction of the Sun for the Earth. We reduce the study of the motion to a case in which a meteor is approaching a stationary Earth with an original velocity which is the *relative* velocity of meteor to Earth—the velocity, in fact, which fixes the radiant. The meteor is acted on during the subsequent motion, firstly by the attraction of the Earth, and secondly by the "disturbing force" of the Sun, which is small even at the distance of the Moon, and which we may neglect altogether for meteors passing near the Earth. Hence we have reduced the actual motion to the case already considered, viz., a meteor approaching a stationary Earth under its attraction only. Applying the results already obtained there may be, after a series of returns, no change in magnitude or direction in the relative velocity with which the meteor approaches the Earth, *i. e.* no change in the position of the radiant, but, owing to the shortening of the time spent near the Earth, the Earth will encounter the meteor a little earlier at each successive return. The orbit of the meteor will gradually change in form ; it will not retain the same shape and revolve round the Sun ; it is the *relative velocity with which it meets the Earth that remains unchanged in direction and magnitude.* If we suppose different members of a swarm unequally affected in this way we can see how the original swarm may be scattered into a swarm which gradually meets the Earth earlier and earlier, but yet gives the same radiant. This is the cause which I suggest as the possible explanation of stationary radiant-points. I do not venture to put it forward as a completely satisfactory explanation. There are two difficulties. First it is a little difficult to see why the swarm should be scattered all round the orbit, and not simply move *en masse.* What we want to establish is some reason for the selection in the action of the Earth of some particular meteor in the swarm to the exclusion of others. I think we may find an explanation by taking in the rotation of the swarm, for to me it seems inconceivable that a swarm could remain extended without some rotation. This rotation may afford the means of giving the Earth a choice among the meteors, so that some are continually trailed round the orbit, and others may remain unaffected in a manner stated in the paper, but too long to mention now. Secondly, my explanation proves too much, as it gives the relative velocity constancy not only in direction but in *magnitude.* Mr. Denning tells me that meteors from fixed radiants vary in velocity—those near the apex move quickly, and those near the anti-apex slowly. Without giving a complete solution of the difficulty, I have indicated how something of an explanation may come from the action of a resisting medium. I confess it does not take us very far, but it explains the difference between the apex and the anti-apex. I hope I have made it clear that the explanation I offer is not yet a completely satisfactory one; but it is put forward in the hope that it may attract the attention of mathematicians to a subject which seems to me to be very important. We have on the one hand Mr. Denning, with 30 years' experience, saying he is not only convinced from the meeting of paths, but from the character of the meteors, that they have the same radiant. On the other hand we have no satisfactory explanation of it from theory. I have looked up, during the last month or two, the attempts to explain stationary radiants. They do not seem to be very serious. I do not think sufficient attention has been given to the matter.

Prof. Herschel. I very much wished to hear Prof. Turner's exposition of this view, because I felt sure that it could not be in better hands. I may say, as regards directing the attention of future astronomers to the question, that he has a very safe prospect, because it is a clear fact that the drawing back or retrovection of the node must occur to the meteors disturbed by the Earth, and must go on occurring for any length of ages, so that transportation of the stationary radiant-point round the Earth's orbit must follow as a dynamical effect. For the theory which Prof. Turner has put forward is really a new mathematical proposition in gravitation, which may not have very great attractions for everybody, but which cannot be obliterated. As to the evidence for stationary radiant-points, there is as much to be said for the observations as for the capabilities of the explanation. I think there is no doubt there are such things as stationary radiants, although in general of a feeble kind. I have not heard of very strong showers being fixed in the sky; it is only by practised observers that they have been detected, and it is not ordinary observers who can find them, but only those who have industriously studied them. Thus it may be the scattered stray disturbances of the Earth upon a shower, carried on for a long time, which have brought some stragglers into the new positions. (Prof. Herschel gave a simple explanation, by means of a ruler on the blackboard, how the node could be shifted by the Earth's attraction.) As regards the velocity objection which Prof. Turner has referred to, I think the fact that the velocities are kept constant in magnitude all the time the radiant-points are carried round, very likely will be found not to conflict seriously with observations. I do not think meteor observations are so exact as to enable one to contradict theory from that point of view. But there are certainly sometimes very anomalous velocities of meteors, which will very possibly be cleared up and explained by the attention which I hope this theory will receive.

Dr. Johnstone Stoney. I have been engaged during the last summer on an investigation into the dynamics of the Leonid swarm, which has brought to light one case in which the cause of a nearly stationary radiant can be fully traced—namely in the case of the sparse shower of Leonids which presents itself every November and lasts for ten or twelve days. Were it not for the compensating causes to which I shall refer, the radiant of this protracted shower would, during the progress of the shower, shift to the eastward some ten or twelve degrees along a parallel of latitude. This annual shower is a shower of clino-Leonids, *i. e.* of the Leonids which describe orbits that sensibly differ from the orbit of the dense stream of ortho-Leonids which visits the Earth three times in a century, and which, while it is passing (which occupies two or three years) produces the greater Leonid displays, each lasting only a few hours and seen only from the side of the Earth which at the time happens to be its advancing side. In contrast to this, the Earth encounters clino-Leonids every year, and on each occasion they are seen for several days in succession and from all parts of the Earth.

Some clino-Leonids have had the origin to which Prof. Turner has referred, namely that they have been diverted by the Earth's attraction out of the main stream; others were originally dispersed by Uranus; and some are Leonids which have strayed owing to having at some time suffered abnormal perturbation.

It can be shown that those which produce our ordinary annual shower belong to the second of these three classes, that they are those Leonids which were dismissed over a wider cone of dispersion than the ortho-Leonids, when (probably in the year A.D. 126) Uranus diverted the whole swarm into the solar system. We may call these the planetary clino-Leonids, as they were made clino-Leonids by the planet Uranus. There are two types of them—those which have since moved in orbits with a greater axis minor than that of the ortho-orbit, and the others whose orbits have a less minor axis. At first, seventeen centuries ago, such of the former type as encountered the Earth entered our atmosphere from radiants that lay to the east of the ortho-radiant, while those of the other type had radiants lying to the west of the ortho-radiant. Since then all the Leonids have been subjected to perturbations from the surrounding planets, which on the average advances the node of the ortho-orbit relatively to the stars about 52″ each year along the ecliptic, and by a nearly equal amount shifts the radiant of the ortho-Leonids to the eastward along a parallel of latitude. Now the dynamical investigation shows that the planetary clino-Leonids of the two types have been differently affected by this perturbation. Those of the first type have had their nodes advanced along the ecliptic more slowly than the ortho-Leonids, and those of the second type more quickly, so that now, after 17 centuries, the Earth encounters Leonids of the first type for some days before the ortho-date, and Leonids of the second type on days after that epoch.

The shift of the node is necessarily accompanied by a nearly equal shift of the radiant, so that while all the radiants are advancing towards the east, they travel at different rates and in such wise that the radiants of Leonids of the first type, which lay originally too much to the east, seem to be slipping backwards relatively to the ortho-radiant, while those of the second type, which originally lay too much to the west, are making their way forwards, thus causing all the radiants to close towards one another and producing what is now an approximately stationary radiant; not, however, quite stationary, since a scrutiny of the observations seems to indicate that there is at present outstanding a shift forwards during the continuance of the shower, amounting perhaps to some two degrees, where, but for the causes to which I have adverted, the shift would have been ten or twelve degrees. The observations are too rough for us implicitly to rely on them. If we may trust them they seem to show that the compensation was complete at a certain date not long past, and that a divergence then began to take place in the opposite direction; if so, it was at that date that the radiant was most nearly stationary.

Prof. Herschel. The neat adaptedness and admirable simplicity of the purely dynamical explanation of stationary radiant-points, presented to the Society by Prof. Turner, moved me to give a short account to the Society, as an accompaniment to his communication, of another suggested explanation, also in part mechanical, but differing in its main outline entirely from Prof. Turner's in that it adopts empirically some leading views intended to explain at once, more directly than by the very gradual effects of perturbations, the possible persistence of a stationary meteor-shower either through a whole year or through any portion of a year. Recourse to those adopted views was suggested by an explanation offered long ago by Mr. Proctor, that there might be very swift meteors, perhaps, among those that are sometimes seen, so swift that the Sun's attraction could scarcely deflect them from their course, and the Earth's passage through the stream would not sensibly affect their apparent radiant-point, so that in any portions of the Earth's orbit where a gust of such cosmical meteors might strike the Earth's neighbourhood, the radiant-point of the whole meteor-shower would appear visually fixed, or at least approximately stationary. But this does not accord well with present observations, as no meteor visitants of such enormous swiftness are ever now observable, which would have to be moving at the rate of at least 600 miles a second to be sensibly unaffected by the Earth's motion with changes of position of their apparent radiant-point.

Mr. Proctor's original view was therefore temporarily abandoned, but it has now recently occurred to me that if such things have happened in the past, when matter was distributed more densely round the Earth's orbit than it is now, some of those on-rushes may have struck the nebula ring in which the Earth is involved, much in the same way as a shooting-star strikes the atmosphere now. Catastrophes like those of a shooting-star, but on a more stupendous scale, may have occurred in the terrestrial nebula when some of these old-time gusts rushed through it, and they may have made meteor-streaks of a colossal description and proportions. These streaks would be formed of matter flung out of the ring with all the varieties of speeds seen commonly in the spark-trains of large meteors, all following in a straight course the direction in which the swift-paced meteor perforated the ring-bodies, or shot through some of the substance of the ring.

But that direction would be the relative one of the gust's encounter with the ring-matter, and we have supposed it to be sensibly independent (by the gust's enormous speed) of either the Earth's or the ring-matter's orbit-motion, so that at all points of the ring or terrestrial orbit whence the spray-stream was flung off, it would be projected in a sensibly permanent relative direction. The crushed and splintered, or volatilized and melted fragments of this spray, after describing all possible forms of ellipses or closed conic sections round the Sun, due to its parts' promiscuous velocities of projection, would, in very various times of revolution, return to the ring-point of first encounter, as shooting-stars, aerolites, and fireballs, or it might even be conceived as comets, on courses of exactly the same directions and velocities as those with which they started. But meeting there with either the Earth or some other substantial member of the ring moving exactly as the parent ring-bodies or masses were, from which the passing gust first stripped away the fragments, no matter what solar courses they may have in the meantime followed, the final arrival courses, as relatively apparent to the Earth or ring, would be exactly the same as those with which they set out, or were torn away from the same bodies by the sweeping cosmical blast in long tails of sparks like that following in the wake of a shooting-star or of a rocket. The radiant-point of these apparent courses would be, for all alike, the fixed or nearly-stationary apparent one of the blast itself, and would thus be found nearly stationary, at whatever part of its yearly course the Earth should happen to encounter any of these meteors.

Of such débris as the blow produces, only the slow-sped portions, perhaps the quarry-masses which descend as meteorites and large detonating fireballs, receive nearly the same elliptic paths with the Earth about the Sun, acquiring thus, as they are in general found to have, direct-moving orbits; and all of the detritus which thus, from slow projection-speeds, pursue short-period ellipses, would return so often in ages which must have elapsed from the remote eras here contemplated of the solar system, that they would have been mostly re-absorbed, in time, by the Earth and by such residual masses as continued, with the Earth, to constitute for a time a terrestrial planetary ring. But of the more rapidly projected portions, thrown into ellipses of such long periods as not to differ perceptibly from parabolas, returns to the Sun would naturally occur so seldom, that they might be reasonably expected to form, as in company with the majority of comets they appear to do, the main surviving bulk of meteor-apparitions. Perhaps some parabolic moving shooting-stars are even now paying a first visit to the Earth after their expulsion from original companionship with an unformed world of masses pursuing terrestrial courses in pre-geologically ancient ages. Still swifter outcasts from the nebular or planetary ring, of which it seems natural to assume that a copious stream would be produced, would never revisit the Earth or Sun at all; but, in more or less open hyberbolic curves about the latter orb, would pursue the roving meteor-cloud into the unknown realms of outer space from which the swift swarm of assailant meteorites swept in, and to which, after a short, perhaps "'temporary star"-like visit to the Sun, it again betakes itself.

Mr. Proctor, it would seem, may thus have been not very much mistaken in imagining, for the substance of his explanation, a "'fifth order of comets or meteor-streams" projected with immense velocities from such "giant suns" as Sirius. But, in place of those most rapid-flighted meteors, it is now suggested here that in reality it may be only the still enduring records of their violent invasions, not the primitive storms of cosmical meteorites themselves, which observations seem more concordantly to show that long-enduring meteor-showers from fixed, or all but stationary radiant-points present, and which may sometimes now resplendently attract our gaze by showering back upon the Earth in comets, meteorites, and shooting-stars.

The President. We are very much obliged to Prof. Herschel for giving us this interesting communication. He speaks of the high velocity that would be necessary to account for what, after all, would be the most simple explanation of the long-enduring radiants, and that is to assume that these shooting-stars were moving with a velocity so great that the Earth's velocity would be a negligible quantity. I think I understood Prof. Herschel to say that 600 miles a second would be necessary. Of course, that is a high speed; but I do not know any reason why a speed so great as that should not exist. We know that there is a famous star (Groombridge 1830) which moves at 200 miles a second, and that it is only the projection of that star's motion on the sphere which we see. An object falling from infinite space into the Sun would arrive there at a pace of upwards of 400 miles a second. The Sun, after all, is a comparatively small star, so we can hardly doubt that there may be celestial objects travelling at speeds which reach the limit Professor Herschel mentions. Therefore the fact that the speed is a very high one does not put it out of the sphere of possibility.

Rev. C. D. P. Davies read a note on an unsuccessful attempt to photograph the corona without a solar eclipse. The apparatus constructed by the author of the note, with only a slight hope of possible success, consisted in effect of two telescopes, one in front of the other. In the principal focus of the first object-glass was a brass circular disc of a size just sufficient to mask the photosphere. Behind the disc was a second object-glass having the disc in one of its conjugate foci and the plate in the other. On some of the plates taken with this apparatus there were slight traces that might be due to coronal light, but in the absence of any detail it was not possible to prove this. Exposures of various lengths had been tried, the most promising being about 15 or 20 seconds. An exposure of 5 minutes had been given to the Full Moon on one occasion, and on this plate there was a comparative absence of surrounding halo.

Mr. Newall. I have been interested in hearing Mr. Davies' statement, because it is an important problem to endeavour to photograph the corona without an eclipse. I am induced to offer a remark, for last year in India I endeavoured to get photographs of the Sun without an eclipse by going to a high altitude so as to get above that part of the Earth's atmosphere which, in virtue of the suspended dust, &c., is most effective in producing the glare round the Sun. The only chances of photographing the corona without an eclipse, with any possibility of success, are such as might be found in the high Alps or the Himalayas at the proper seasons. The question is, whether one is able to get enough contrast between the light scattered by the terrestrial atmosphere alone, and that light plus the coronal light. It is simply a contrast between those two very nearly equal quantities, and the best chance of being able to show the existence of contrast of that kind is to go to a place where the intensity of the light reflected from our atmosphere is greatly reduced in comparison with the coronal light. I used a special pinhole camera, by which I hope to get some result. After waiting at Darjeeling for four days, on which there was bright sunshine (accompanied, however, by glare that produced such a disturbing effect that it was useless to take photographs), a most brilliant day came when I thought I should have had some success. If one put up a finger to block out the Sun, one could look without any discomfort at its neighbourhood. I was then in hopes of being successful, but the results of the photographs now lead me to believe that it is almost a hopeless problem.

Mr. Newbegin. It has often crossed my mind that we shall have to put up the spectroscope to achieve the object of photographing the corona. Already the Sun, with features on its surface, has been photographed by making a dual slit move across the different ends of the spectroscope, and I have often wondered whether it would be possible to so rotate all the lines of the spectrum that one might get not only the lines which reach the solar flexures, but the lines which reach the coronal flexures, and thus photograph both Sun and corona.

Rev. C. D. P. Davies. The last speaker says that he looks to the spectroscope to carry out this object. I have always looked to the spectroscope; but this is only a preliminary skirmish which, I may say, was due to the successful photograph taken of the Sun by Mrs. Maunder.

Mr. Dyson said that he had one slide which the Astronomer Royal had asked him to show. Some photographs had been taken on six nights of Neptune's Satellite. In the photograph shown on the screen there were three images, taken with exposures of 5, 4, and 3 minutes with the 30-inch reflector of the Thompson Equatorial. The images of Neptune were slightly elongated, but this was due to defective driving. The satellite was shown distinctly at about a radius distant from Neptune. It would be possible to photograph it in different positions round Neptune. Three of the plates had been measured, and they seemed to be pretty good. Another photograph was exhibited of the Andromeda nebula which showed a very fair amount of detail.

The President. I think Greenwich Observatory should be congratulated upon these results, which show that we are not behindhand in astronomical photography, and promise that we shall be fully able to hold our position.

Mr. Maw here took the Chair.

Rev. J. M. Bacon then read a paper on the value of possible observations from free balloons. During the past summer and autumn he had made a series of balloon ascents, seven in all, under varying conditions at different hours of the day and night, the principal object being to gain information as to the state of the atmosphere at different heights, and also with the idea of securing observations. The purpose he had in view was to determine how much more of the solar spectrum could be photographed at a great height, more particularly the extension of the ultra-violet, secondly to attempt to observe the solar corona, and thirdly to undertake such observations as could be usefully made with low power. (Photographs in illustration of the ascents were thrown on the screen and described by Mr. Bacon.)

Prof. Turner. I do not know if I am right in gathering from the paper that, although there are immense difficulties in the way of success, Mr. Bacon is able to formulate conditions and to say under what circumstances the conditions are favourable, and under what circumstances they are not.

Rev. J. M. Bacon. I have endeavoured to determine whether these conditions depend largely on the amount of dust in suspension. I have taken up an Aitkens dust-counter on several occasions, but I have had no success with it. I have tried other methods to find out how much dust there is in suspense, but I have not proved that what dust there is is a factor in the case.

Dr. Spitta. Do you know anything about the cause of tremulations? In Switzerland I have noticed tremulations which might be in consequence of the heat of the Earth. Did that disappear at 6000 feet? Also I would like to ask whether the photographs taken from the balloon were all taken with the camera directed to the same point of the compass? If this were so, they are hardly comparable, for the dissimilarity on the earth immediately under the car is not remarkable, if a picture taken looking North is compared with one taken looking South. The haze then is due to the Sun coming into the camera.

The Chairman. Were there any layers of different densities, alternately hotter and colder, and more moist and less moist?

Rev. J. M. Bacon. There were not on these occasions. The temperature would decrease gradually and not to a great extent—not as at night. We did not find that it fluctuated up and down. We did not get into pools of warm air, and then again into cold air. In this respect there was a gradual decrease.

The Chairman. The paper shows that there are many possibilities in balloon investigation, and I think, if Mr. Bacon continues his investigations, as he intends, I believe, to do, I have no doubt we shall hear from him many interesting notes giving the results of his further experience.

The following papers were announced and partly read:—

H. H. Turner. "A Suggestion for the Explanation of Stationary Radiant-points of Meteors."

A. S. Herschel. "Remarks on Prof. Turner's Paper, with another suggested Explanation of Stationary Radiant-points of Meteors."

Perth Observatory, W. Australia. "Observations of the Leonids, 1898 November."

Rev. J. M. Bacon. "On the Value of possible Observations from Free Balloons."

H. H. Turner. "Note on Dr. Gill's Paper 'On a new Instrument for Measuring Astrophotographic Plates.'"

C. D. Perrine. "Note on Mr. Espin's Object in Perseus."

Rev. W. Sidgreaves. "Eclipse of the Moon, 1898 December 27."

W. H. Robinson. "The Great Sun-spot of 1898 September."

Rev. C. D. P. Davies. "Note on a preliminary and unsuccessful Attempt to Photograph the Corona without an Eclipse."

W. E. Plummer. "Occultations of Stars during the Lunar Eclipse of 1898 December 27."

Royal Observatory, Greenwich. "Observations of Occultations of Stars and Planets by the Moon and of Phenomena of Jupiter's Satellites in the Year 1898."

T. W. Backhouse. "Observations of the Brightness of α Orionis, 1895–98."

Royal Observatory, Greenwich. "Note on Photographs of the Satellite of Neptune taken with the 30-inch Reflector and 26-inch Refractor of the Thompson Equatorial."

The following gentlemen were elected Fellows of the Society:—

Charles L. Brook, M.A., F.R.Met.Soc., Harewood Lodge, Meltham, Huddersfield.

Arthur Hands, L.R.C.P., M.R.C.S., Inkerman House, Wednesfield Road, Wolverhampton.

Samuel Henry Harrison, F.R.G.S., Frederick Road, Edgbaston, Birmingham.

Arthur R. Hinks, B.A., Observatory, Cambridge.

Worcester R. Warner, Cleveland, Ohio, U.S.A.

The following Candidate was proposed for election as a Fellow of the Society:—

Col. Thomas Davies Sewell, Clerk to the Spectacle Makers' Company, 29 Grosvenor Road, S.W. (proposed by Sir R. S. Ball).

THE BRITISH ASTRONOMICAL ASSOCIATION.

THE second ordinary Meeting of the Session was held at Sion College on Wednesday, December 28, *Mr. W. H. Maw*, President, in the Chair.

The election of twenty new Members was announced, twelve of whom belonged to the two Scotch branches.

The Rev. A. L. Cortie read a paper, illustrated by numerous lantern-slides, on "Some Problems with regard to Faculæ." Father Cortie remarked that the first question was the order of the appearance of faculæ relatively to the spot outburst which it accompanied. There could be no doubt, from a consideration of the Stonyhurst drawings, that, as regards the extent of area covered and the duration, the faculæ followed the sun-spots. Where a new spot appeared among the faculæ, the contrary impression might be produced if the life-history of the spot was not known. The question could only be properly studied at times of minimum solar activity. The supposed lagging of faculæ was not borne out by careful observations. From the cases so far studied at Stonyhurst, the rotation period of faculæ was in accordance with Carrington's law for spots. Attention was then called to the seeming shadows which were sometimes seen outlining the edge of a bank of faculæ. No satisfactory explanation of this phenomenon had yet been suggested. The last point touched upon was the reversals in the H and K lines, which were to be seen in the spectra of a spot and the surrounding faculæ. A re-reversal of the bright line, which gradually became broader as it extended from the single reversal over the spot, occurred in the faculæ.

Mr. Maunder said they were very much indebted to Father Cortie not only for the exhibition of the beautiful drawings of sun-spots which had been thrown upon the screen, but also for introducing the subject just at the present time. There was always a danger lest solar observers should slacken in their work during the time of minimum, but there was no doubt, as had been just pointed out, that there were several problems that were really best attacked at that phase of the solar cycle. His own experience was confined mainly to the study of photographs of the Sun, and in dealing with the question of faculæ this was a disadvantage, as it was doubtful whether the faculæ were brought out on the photograph as clearly as they could be picked out on the Sun itself. So far as his recollection went, faculæ did not precede spots, but that might simply mean that the photograph did not show them. He had often noticed the so-called shadows, and in measuring spots it was often a difficulty to know what to do with them. Sometimes they might be regarded as simply contrast effects, and other times they were too dark to be so explained and were classed as "faint diffused" or "nebulous" spots.

Miss Brown hoped that the members would be encouraged by Father Cortie's address to pay more attention to faculæ. A good many members of the Solar Section had undertaken their study, but those who could not measure their positions found it rather difficult to carry out observations of that kind.

Capt. Steele asked whether the spots on the illustrations shown by Father Cortie had been darkened by artificial means, or whether they were simply as recorded on the photographs.

Father Cortie said the illustrations thrown on the screen were not photographs, but drawings on ground-glass. They exactly represented, however, what was on the original drawings at Stonyhurst, so that the spots on the originals were as dark as those shown that evening.

Mr. Holmes said that perhaps Father Cortie would explain why he was so confident that the dark markings were not the effect of contrast.

Father Cortie replied that there certainly was an effect of contrast between faculæ and the photosphere in their neighbourhood. When they observed faculæ it was always towards the limb, where the photosphere had a darker appearance, since they were looking obliquely through a greater depth of the Sun's atmosphere. But the markings shown were so much darker than that contrast effect that he could not suppose them to be mere contrast. With regard to the point raised by Capt. Steele as to sun-spots being vortices in the solar atmosphere, that was a theory which several solar observers had held, in particular M. Faye, who considered that they were exactly analogous to typhoons in the atmosphere of the Earth. With regard to sun-spots being an uprush or downrush he did not hold any theory on the point at all.

Dr. Johnstone Stoney announced that he had received a letter from Prof. Barnard on the subject of the Leonid shower. Before reading the letter Dr. Stoney reminded his audience that this meteor-stream was known to consist of a main stream of meteors that might be called the ortho-meteors and a second class that reach the Earth in a more scattered form, and which present themselves every year. The front of the main stream reached the Earth this year, and what had been observed in America was the front of the main stream of ortho-Leonids. It appeared from Prof. Barnard's letter that he commenced his watch on November 11, and continued it until November 16, and also in the latter hours of November 17, from 5 or 6 P.M. till 6 or 7 A.M. each night. The sky was clear on the night of the 14th, and from midnight till daylight several hundred meteors were seen, many of the first magnitude and a few brighter. Few were seen near the radiant, their average position of distance from the radiant being 90°. Their flight was very rapid and the maximum of the shower was reached between 3 and 4 A.M.* An attempt to photograph trails had failed. In reply to the President, Dr. Stoney said he had not seen any reports from more western stations, but that at Harvard they had been successful in getting a number of trails photographed, an extremely important thing, because it would make it possible to fix with accuracy the radiant-point of the front of the stream.

Mr. Crommelin mentioned that the photographic search for the meteoric radiant had a valuable bye-result, namely the discovery of Chase's Comet.

Mr. Maunder said that he understood that Prof. Pickering had on his photographs four instances of the same meteor-trail being photographed at two places, which would of course give the height of the meteor.

Dr. Stoney said that he exceedingly regretted that neither he nor anyone else had thought of computing beforehand the direction in which the stream might be seen end-long just as it passed. It would have been exceedingly interesting to have turned a camera in that direction. Unfortunately, owing to moonlight, there would be very little prospect of securing a photograph of it in any of the other years when the Earth would pass through the stream.

Mr. Salmon exhibited a number of slides illustrating "An Attempt to Counterfeit Lunar Formations." He remarked that the slides hardly did the subject justice, as it was his first attempt at micro-photography.

The President remarked that some of the slides shown might very well have passed for lunar photographs, and there was no doubt that many would not have discovered that they were artificial but for Mr. Salmon's explanation.

Mr. Schooling read a paper on the work of an Astronomical Folk-Lore Department, the idea of which was to collect myths and folk-lore, and to record primitive ideas of astronomy among people who were emerging from the mythical state.

The Rev. A. L. Cortie did not see where they were to draw the line, and suggested as an alternative the investigation of the beginnings of the history of astronomy.

Mr. Maunder thought Father Cortie's suggestion a valuable one and believed that they would find such work suggestive and directly useful.

The President said that from the remarks that had been made he judged that the meeting was not prepared to back up Mr. Schooling's suggestion.

He then announced that *Prof. C. V. Boys* had kindly undertaken to deliver an address at the next meeting of the Association, to be held on January 25, on "The Determination of the Newtonian Constant of Gravitation."

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* 5 hours slow on G.M.T

ROYAL METEOROLOGICAL SOCIETY.

The Annual Meeting of this Society was held on Wednesday evening, January 18, at the Institution of Civil Engineers, Westminster, *Mr. F. C. Bayard, LL.M.*, President, in the Chair.

The Council in their Report stated that owing to the premises now occupied by the Society, at 22 Great George Street, being required by the Government, they had been obliged to seek accommodation elsewhere; but not being able to secure offices in the immediate neighbourhood, they had taken a suite of rooms at 70 Victoria Street.

Mr. Bayard, in his Presidential Address, gave an account of the Government Meteorological Organizations in various parts of the world. He first briefly described the founding of each system, and mentioned the names of the various directors, and then enumerated the number of observing stations associated with each organization. In most countries forecasts of the weather are issued, and Mr. Bayard gave some interesting particulars as to the success attained by each office. The amount of money voted by the various Governments for the support of Meteorology showed what a very small portion of the revenue of the different countries goes towards the promotion of this science. In the British Isles it is two shillings and sixpence per square mile, or only about one-third of a farthing per head.

The Address was illustrated by a large number of lantern-slides, showing views of the various observatories, and portraits of the Directors.

Mr. Bayard was re-elected President for the ensuing year.

The Wear of Micrometer-screws.

In the last issue of 'The Observatory,' in the course of the report of the Meeting of the Royal Astronomical Society held on December 9, you record the discussion on Dr. Gill's paper on "The Effect of Wear on Micrometer-screws," and quote (on p. 40 *ante*) the remarks made by Prof. Turner explaining the illustration of the effect of such wear given by Dr. Gill some years ago. Now this illustration of Dr. Gill's not only explains very clearly how this wear causes trouble, but—it appears to me—also indicates how this trouble can be, to a very great extent, avoided.

Dr. Gill supposes the thread of the screw to be unwrapped, so that it forms the inclined surface of an inclined plane, and he represents the nut by a block moving on this plane. Thus in Fig. 1 (which is practically a reproduction of a sketch made by Prof. Turner on the blackboard) the line AB represents the uncoiled thread of the screw, and the block C the nut. If, now, the nut be moved sufficiently long to and fro for varying distances over a short length of the surface AB, wear will take place, and the state of affairs shown by the dotted lines will result. Under these circumstances the screw, instead of having a uniform gradient, or pitch, from end to end, will have between the points D E a pitch which is flatter than the mean, and between the points E F one which is steeper. It is this irregularity of wear, and consequent alteration of pitch in different parts of the screw, which occasions trouble.

Fig. 1.

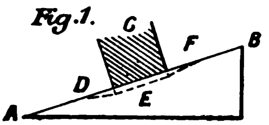

Fig. 2.

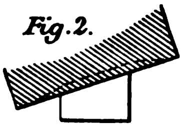

Fig. 3.

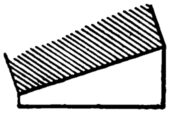

Fig. 4.

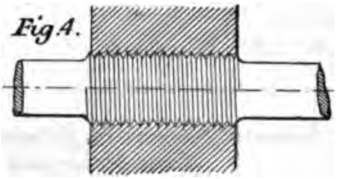

Fig. 1 represents the action which occurs with the ordinary proportions of screw and nut; but it is evident at once that the effect may be materially modified by varying these proportions. Let us, for instance, consider the effect of making the nut longer than the screw, as indicated in Fig. 2. Here the conditions will be reversed, the wear taking place as shown by the dotted lines, and the surface of the inclined plane instead of being worn hollow becoming convex.

Between these two extremes there is evidently a proportion of length of nut to screw which will produce neither concave nor convex wear, but will cause the surface of the inclined plane to wear away without modifying its inclination. This result will, it appears, be secured by making the nut the same length as the screw, as shown diagrammatically in Fig. 3, and in Fig. 4. Any shift of the nut from the middle position will thus cause the nut to overrun the screw at one end or the other, and the nut must be made of such a length that an ample number of threads remain in gear with the screw when the latter has been rotated its maximum number of turns from its middle position.

As regards the length of the nuts of micrometer-screws, they are in general made much too short; but even with these short nuts I believe it would be found an advantage to turn off the threads of the screw, as shown in Fig. 4. Generally, however, there is no real reason why nuts should not be made much longer, and the wear will be found to vary almost inversely as the length.

In the course of the discussion on Dr. Gill's paper reference was made to the effect of reversing worn screws, and from certain remarks it appeared as if the effect of such reversal was not in all cases properly appreciated. A micrometer-screw generally acts against a force exerted constantly in one direction, and thus one face only of the screw becomes worn in the manner indicated in Fig. 1. If, then, the screw be reversed, a new surface with a uniform gradient is brought into action, and matters are restored to their original condition, save in one rather important respect, namely, that the new surface, instead of being opposed to a properly fitting nut, acts upon a nut which has a thread worn to correspond with the rounded face shown by the dotted lines in Fig. 1. This will cause the wear of the reversed screw to be more rapid than that of the original screw, and we see that to obtain the best results both screw *and nut* should be reversed. By this means matters would practically be restored to the conditions first existing.

Kensington, 1899, Jan. 9. W. H. Maw.

Meteoric Showers in Autumn, Winter, and Spring from Ursa Major and the Region near.

The well-known and tolerably bright stars of Ursa Major afford an excellent means for the accurate determination of such meteoric radiants as are distributed amongst them and in the outlying region towards the Lynx, Draco, Boötes, Canes Venatici, and Leo Minor. These radiants are very numerous, and many of them appear to be sharply defined. I lately compared those of my meteor-paths which were apparently directed from Ursa during the period from September to April, and drew up the following summary of the principal radiant-points. My observations, however, have not been sufficiently numerous during the first four months of the year to enable me to detect the more feeble streams in action during that period. In the autumn season I have pretty fully ascertained the radiants in this region from a considerable number of shooting-stars, and I believe the positions included in the first column may be relied on. Some of these autumn showers seem to recur or continue in the winter and spring months from nearly the same positions, but the two latter periods require much further investigation. As supplementing the results of my own observations I have appended a table of similar radiants which I derived by the projection of a large number of meteors in the published catalogues of the Italian Meteoric Association, of Heis, Zezioli, Weiss, Tupman, Denza, and others. The two lists appear to be in fair accordance. It will be well if, in future years, observers devote more attention to these interesting Ursid showers. They are nearly all circumpolar and furnish meteors contemporary with the Orionids (Oct. 18–20), Leonids (Nov. 15), Andromedes (Nov. 23), Geminids (Dec. 10–12), Quadrantids (Jan. 2), and Lyrids (April 20).

The most prominent of them all in the autumn is that at 154° +40° (near μ–λ Ursæ Maj.). It is very rich in November and supplies many bright, swift meteors with streaks, and very similar in aspect to the Leonids. There is also a fine shower in the N.W. borders of the constellation, at 134° +68°, near σ, which becomes well defined in September, and apparently lasts right on through the winter and until the April ensuing.

Meteoric Showers in Autumn, Winter, and Spring from Ursa Major and the Region near, derived from Observations at Bristol.

Autumn.			Winter.			Spring.		
Radiant.	Meteors.	Period.	Radiant.	Meteors.	Period.	Radiant.	Meteors.	Period.
α. δ.			α. δ.			α. δ.		
125° +40°	21	Oct.-Dec.	......	...	....	......	...	....
125½ +56	23	Sept.-Nov.	126 +57	7	Jan.-Feb.	......	...	....
130 +47	25	Sept.-Oct.	......	...	....	......	...	....
133 +43	9	Nov.-Dec.	130 +44	11	Jan. 14-25.	......	...	....
134 +68	49	Sept.-Nov.	134 +67	10	Jan.-Feb.	......	...	....
......	...	....	140 +57	6	Jan.	......	...	....
143 +49	36	Oct.-Dec.	141 +50	4	Jan. 17-25.	142 +50	11	Mar.-April.
154 +40	74	Sept.-Dec.	156 +41	6	Feb. 2-20.	156 +40	7	Mar.-April.
157 +49	5	Nov. 14-23.	......	...	....	......	...	....
161½ +58	21	Nov. 30-Dec.	......	...	Jan. 19-27.	161 +58	20	Mar.-April.
167 +33	22	Nov.-Dec.	168 +33	3	....	......	...	....
......	...	....	174 +56	8	Jan.-Feb.	......	...	....
177 +49	12	Dec. 28-29.	179 +47	9	Jan. 17-21.	177 +47	7	Mar.-April.
190½ +58	23	Nov. 13-Dec.	......	...	....	190 +58	13	Mar.-May.
194 +67	24	Nov. 14-Dec.	......	...	....	196 +67	7	Mar.-April.
194 +43	13	Nov. 13-Dec. 4.	......	...	....	195 +44	15	Mar.-April.
194 +32	7	Dec. 18-29.	......	...	....	193 +33	15	Mar.-April.
201 +57	13	Nov. 15-Dec.	......	...	....	204 +58	9	Mar.-April.
208 +43	17	Nov.	......	...	....	......	...	....
....	...	....	213 +53	5	Jan. 20-25.	213 +53	10	Mar.-April.

Similar Showers determined from the Meteor-paths registered by other observers.

Autumn.			Winter.			Spring.		
Radiant.	Meteors.	Period.	Radiant.	Meteors.	Period.	Radiant.	Meteors.	Period.
α. δ.			α. δ.			α. δ.		
120° +40°	9	Nov. 1-7.	122° +46°	13	Jan.-Feb.	123° +40°	19	Mar.-April.
123 +57	10	Sept. 24-26.	......	...	....	......	...	....
134 +49	37	Nov.-Dec.	135 +48	6	Jan. 1-15.	......	...	....
132 +66	11	Nov.-Dec.	133 +66	15	Feb.	134 +67	30	Mar.-May.
144 +52	14	Nov. 1-15.	145 +55	5	Feb.	......	...	....
153½ +42	29	Sept.-Dec.	150 +43	7	Jan. 1-15.	153 +45	19	Mar.-May.
......	...	....	157 +60	21	Jan.	162 +59	24	Mar.-April.
160 +40	8	Oct.-Nov.	164 +35	5	Jan. 1-15.	162 +34	23	April-May.
178 +46	10	Dec. 9-12.	......	...	....	......	...	....
189 +57	8	Oct. 29-Nov.	183 +54	26	Jan.-Feb.	184 +60	51	April.
......	...	....	195 +62	5	Jan. 1-15.	......	...	....
......	...	....	200 +58	7	Jan. 1-15.	......	...	....
204 +43	13	Nov. 13.	204 +45	8	Jan. 1-15.	207 +45	67	Mar.-May.
216 +49	5	Sept.	205 +51	7	Feb.	215 +54	15	April.

Bristol, 1898, Dec. 21.

W. F. Denning.

Prof. Newcomb's Planetary Tables.

THE publication of Newcomb's "Tables of Mars" ('Astronomical Papers for the American Ephemeris,' vol. vi. part iv.) completes the series for the four inner planets. The form of all the tables is substantially similar. The elements and constants are first given, together with certain empirical corrections that have been applied. Then follow the inequalities depending on the first order of the masses. Abstraction is made of all inequalities whose periodic time is that of the planet considered. Their coefficients, when determined by observation, are merged into the elliptic inequality, and it seems natural to so merge them in the tables. In doing this, however, Newcomb is not following Le Verrier. Next come the inequalities of long period, many of them involving the product of the masses of two or more planets. The rest of the various parts is devoted to the tables and explanations of their construction and precepts for their use.

Newcomb has not succeeded in completely reconciling observation with theory. The chief discordance is in the motion of the perihelion of Mercury. To reconcile theory with observation Newcomb has supposed a resisting medium, the effect of which is to produce a secular motion of the perihelion proportional to the mean motions. Of course, when such a correction is applied to Mercury, consistency requires a similar correction for the other planets. The other eccentricities, however, are so small that it is difficult to obtain the motion of the perihelia from observation with sufficient accuracy to test the empirical corrections. The long period inequalities have also proved troublesome. The shorter inequalities are churned out by a process that leaves nothing, except perhaps brevity, to be desired; but those of longer period are calculated by approximate methods, and it is by no means certain that all the sensible inequalities have been discovered.

The convenient arrangement of the tables is easily appreciated. To calculate a single place by the tables requires perhaps only one quarter of the labour that the same operation would require with Le Verrier's tables, and the gain is still greater in computing an ephemeris; for whereas Le Verrier divides the circumference of the circle into a round number of parts, Newcomb constructs each of his arguments so that they increase by unity in a day. A planet's place is a function of a single argument—the time; but this fact is disguised by the fact that the perturbations depend both upon its own mean anomaly and that of the disturbing planet. Le Verrier's tables therefore require the calculation of two arguments for each epoch. Newcomb's tables, however, by a device that is at least as old as Hansen's tables of the Moon, take as the second argument the position of the disturbing body at the last perihelion passage of the disturbed. The tables, though in form of double entry, are thus in practice reduced to single entry.

P. H. COWELL.

Witt's Planet, (433) *DQ**.

A CAREFUL search has been made by Mrs. Fleming upon the Harvard plates for early photographs of Witt's Planet, (433) DQ. Mr. S. C. Chandler has courteously furnished ephemerides based upon the best available material, and has devoted much time to correcting the elements and computing the positions corresponding to the times at which certain photographs were taken, as is more fully explained in the 'Astronomical Journal,' No. 452.

In making this search the following method of procedure has been adopted. Mr. Chandler, by means of the elements published in the 'Astronomical Journal,' No. 451, computed ephemerides for the oppositions of 1894 and 1896. It appeared that the observations then available were insufficient to determine the position in 1894. An error of 1″ in the mean daily motion in the orbit would change the right ascension of the object in 1894 by about half an hour. Moreover, the value of the daily motion differed by several seconds not only in the early ephemerides of this planet, but in those dependent on a large number of visual observations. Although plates were examined by Mrs. Fleming, covering a region of about 1300 square degrees, the planet was not found. Plates taken in 1896 were next examined, as it was thought that the smaller errors of the ephemeris would compensate for the extreme faintness of the planet. This examination proved to be especially laborious and fatiguing to the eyes. It was feared that the object might be too faint to appear upon the plates and accordingly the faintest objects were carefully scrutinized.

Each plate was examined by superposing it upon another plate of the same region taken with the same instrument. Two adjacent images then appeared of each star, while the planet, if present, would appear only upon the upper plate. Numerous suspicious objects were thus found, including several images of the planets Flora (8) and Nysa (44), and two new variable stars, whose approximate positions for 1900 are in R.A. $8^h\,33^m{\cdot}9$, Dec. $+50°\,29'$, and R.A. $18^h\,38^m{\cdot}7$, Dec. $-38°\,52'$, were discovered. The star $+29°551$ fails to appear on ten plates which show other faint Durchmusterung stars, and two stars at R.A. $9^h\,11^m{\cdot}4$, Dec. $-10°\,43'$, and R.A. $9^h\,11^m{\cdot}5$, Dec. $-10°\,16'$, appear upon the Durchmusterung charts and upon the photographic plates, but are not given in the Durchmusterung catalogue.

At last a faint image was found on a plate taken on June 5, 1896, and confirmed by other plates taken on June 4 and June 5. A plate taken on April 6 covered the region of the planet which was readily found by means of its computed position. Mr. Chandler, from positions of these images, was enabled to furnish a corrected ephemeris for 1894, by means of which the planet was readily detected on several plates.

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* This account of the detection of the new planet referred to in our Minor Planet Notes, p. 102, is extracted from 'Harvard College Circular' No. 36.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Cause of the Gegenschein.

GENTLEMEN,—

If my letter in the November number of 'The Observatory' has been the cause of eliciting from Mr. Evershed his most ingenious hypothesis to account for the appearance of the Gegenschein, its publication was, I think, amply justified. Although he has termed his supposition "outrageous," it seems to be well worth careful consideration. I should prefer, however, to withhold my assent until—as medical men would say—simpler measures fail. If I may be permitted to say so, I think my pilll is easier to swallow! Escapes of gas are sometimes dangerous to trifle with! If Mr. Evershed thinks my hypothesis too "shadowy," his substitute for it has, admittedly, very little "substance" in it!

To pass from theory to observation, I fear that more than one phenomenon has been called the Gegenschein. My observations fully confirm the statement of Mr. Evershed that the Gegenschein was unusually distinct on November 5 last, for although I have often looked for it I never certainly saw it until the evening of November 6, and it was again very distinct on November 7. But I think there can be no doubt whatever that at least a portion of the light which he has called the Gegenschein was the zodiacal band; for on the evenings of the 6th and 7th of November the band extended from the apex of the western cone of light right along the ecliptic to τ Tauri, and it must also have done so on November 5, which, however, was a cloudy night here.

On the night of November 6, at 11 P.M. (about $12^h\ 8^m$ Greenwich time), I noticed "a brighter patch on the band, about 4° or 5° in diameter, circular, or perhaps elliptic in shape, its centre being, as near as I could judge, about longitude 46°, and about on the ecliptic. I had no idea whatever at the time what the Sun's longitude was," but I afterwards found that I had placed the centre of the Gegenschein about $1\frac{1}{2}°$ east of the anti-Sun. On November 7, at 9 P.M. (about G.M.T. $10^h\ 8^m$), I mapped the outline of the Gegenschein on a tracing from Klein's star atlas, and found its centre to be about longitude 45° (within half a degree of the longitude of the anti-Sun), its length in the direction of the ecliptic being 6°, and its breadth at right angles to the ecliptic about 5°. The fact of its lying on the zodiacal band might well account for the ellipticity; but I must admit that I was unable to detect any annular appearance in it. It should be borne in mind, however, that if the matter of which the zodiacal band consists extends out beyond the apex of the Earth's shadow, the centre of the Gegenschein (on my hypothesis) would be brighter than the sky, as it would still be crossed by the central and brightest portion of the band if the latter lay on the ecliptic, as it did on November 6 and 7 last. Only upon the supposition that the matter comprised in the zodiacal band did not extend as far into space as the shadow of the Earth, would the centre of the Gegenschein be as dark as the sky outside.

Yours very sincerely,

Madeira, 1899, January 16. WM. ANDERSON.

GENTLEMEN,—

Mr. Evershed's ingenious theory of the cause of the Gegenschein induced me to look for any observations that might confirm or refute it. My astronomical library is not a large one, and I was too much occupied otherwise to give it a complete examination, but, as far as I read, I was surprised at the small number of English observations on the subject. The earliest notice that I met with was by Mr. T. W. Backhouse, F.R.A.S., in the 'Monthly Notices' for 1876, in which the author was evidently unaware of any previous observations. Col. Tupman had indeed referred to it some years earlier, but only on the authority of a distinguished continental astronomer whom he does not name ('Monthly Notices' for 1872).

More than one writer on the subject of the Zodiacal Light describes it as extending across the entire sky and thus passing through the Gegenschein. It is said to have often been thus seen within the tropics. But the astronomer referred to by Colonel Tupman noticed the Gegenschein as an unusually brilliant part of this continuous circle of light. The connection with the Zodiacal Light if established would prove a difficulty in the way of Mr. Evershed's theory, although even on that supposition we should be bound to seek for a cause of the unusual brightness of this part of it. I find, however, that Mr. Backhouse and some others place this bright patch directly opposite to the Sun, whereas Mr. Evershed places it at some 3° from what I may perhaps call the ante-helion. Professor Barnard, to whom Mr. Evershed refers, makes the deviation much less than 3°. I have not his original papers, but summaries of them will be found in the 'Observatory' for December 1891, January 1894, and August 1897. Some of the features noted by him seem scarcely reconcilable with Mr. Evershed's theory. I possess a pamphlet published by the late Mr. Jacob Ennis, in 1878, on the "Electric Constitution of the Solar System." Mr. Ennis does not appear to have been aware of the Gegenschein, but speaks of the Zodiacal Light as spanning the whole of the heavens in the tropics. His pamphlet begins, "The Zodiacal Light, the Aurora Borealis, the corona of the Sun and the tails of comets are all different forms of the same thing. They are electrical brushes precisely the same as the electrical brushes which in the night are seen to fly off from a highly charged electric machine." They would naturally point away from the Sun as the repelling body. I have another curious pamphlet by a Mr. William Daumar, published at Brooklyn in 1887, in which he describes the tail of the Earth as the abode of spirits, and in his peculiar language says:—"Part of the heat which comes from the Sun is fixed to the Earth by chemical and latental processes mainly: it swells a portion of the Earth and gradually creates that zeronic mass of small individual bodies which forms the tail of the Earth. With the zeronization of every organic body a small particle of substance is taken from the globe and added to the tail. The effect of the Sun's heating influence is then to reduce the globe and enlarge the tail of the Earth." Mr. Daumar, I may remark, is not complimentary to the spirits who occupy this region, but he endeavoured to obtain the dimensions of the tail by observing the rates at which they could travel, and obtained a minimum length of 300 miles, which he regards as too small, because they could travel faster in the upper regions of the atmosphere than from New York to Boston, which was his trial course.

With regard to the Gegenschein, besides Professor Barnard's suggestion as to an atmospheric cause of the apparent lagging, it occurs to me that there may be a cause dependent on the velocity of light—light which has to travel first from the Sun to the reflecting particles, and then from these particles to the Earth, taking a longer time to reach us than direct sunlight. The lagging appears to be very slight. Its amount, if accurately ascertained, might (if my suggestion be correct) give us the distance of the reflecting particles. Truly yours,

W. H. S. MONCK.

P.S.—I may perhaps be permitted to suggest the name *Proxima* for the new planetoid DQ. It would at all events mark the state of Astronomy at the time, even if a nearer planetoid should be discovered hereafter.

Dublin, 1899, Jan. 16.

Stars visible to the Naked Eye.

GENTLEMEN,—

I was recently much surprised at reading the following passage in Prof. Maspero's 'Dawn of Civilization' as translated by Mr. M. L. McClure, 3rd edition, p. 205:—

"The Egyptians did not therefore need special instruments to ascertain the existence of a considerable number of stars which we could not see without the help of our telescopes; they could perceive with the naked eye stars of the fifth magnitude, and note them upon their catalogues." It did seem strange that this should be thought a remarkable achievement, but it is apparently founded upon a misunderstanding of a passage in Biot. The following footnote is affixed to the above passage:—"Biot, however ('Sur un calendrier astronomique et astrologique trouvé à Thèbes en Egypte,' p. 15), states that stars of the third and fourth magnitude 'are the smallest which can be seen with the naked eye.' I believe I am right in affirming that several of the fellahin and Bedouin attached to the 'service des Antiquités' can see stars which are usually classed with those of the fifth magnitude."

As most of us in this country can see without any difficulty on a fine night stars of that magnitude (very few of those in Cancer exceed it) and even fainter in a really clear and moonless sky, I was curious to refer to what Biot had really said. The paper in question is in the twenty-fourth volume (for 1854) of the 'Mémoires de l'Académie des Sciences de l'Institut de France,' p. 549, and the passage at p. 563. He is speaking of stars observed in Egypt under certain conditions and for a special purpose, and separating these into four divisions, the last of which relates to stars from the third to the fourth magnitude, "qui sont les plus petites que l'on puisse supposer observées à la vue simple." It is therefore no question of contrasting an Egyptian with a European sky; and though no doubt they have clear skies oftener than we do, it does not seem that more stars can be seen there with the naked eye than we can when the sky is really clear, and when Piazzi thought Greenwich (from personal experience in 1788) a paradise for an observer.

Yours faithfully,
W. T. Lynn.

Blackheath, 1899, Jan. 17.

Confirmed or New Variable Stars.

Gentlemen,—

From my observations I can report the following orange stars, which occur in Espin's 'Revision of Birmingham's Catalogue of Red Stars,' as certainly variable. They are not in Chandler's 'Third Catalogue of Variable Stars,' nor yet in Birmingham's. Espin marks the first two as "Var.?"

1. Es. Bm.	2.	3. R.A. 1890.	4. Dec. 1890.	5. Mag.	6. Average colour.	7. Spectrum type.
		h m s	° '			
598	Sagitta.	19 16 47	+17 27	9·5	ROr^{4}	?
608	Cygnus.	19 25 32	+45 49	8·6	ROr$^{4\cdot2}$	IV.
706	Cepheus.	21 23 1	+62 6	8·8	Or$^{3\cdot2}$	IV.?

Col. 5 gives the B.D. magnitude; col. 6, the average colour from my observations, the notation being the same as that in my letter on p. 278, in July 1897 number of 'The Observatory'; col. 7 gives the type of spectrum as given by Krüger in his 'Catalog der farbigen Sterne.'

Es. Bm. 598 is too faint to tell its colour exactly

The period of Es. Bm. 608, if regular, is probably about 1 year. Variation slight—about ½ magnitude.

Es. Bm. 706 (near α Cephei) has an irregular period averaging about 11 months. Maximum about July 1898. Range about ¾ mag.

The range of Es. Bm. 598 appears to be the greatest of the three.

A remark is quoted in the 'British Astronomical Association Journal' for November, p. 40, that the greatest variation detected by R. O'Halloran in the variable star Bm. 277 Virginis has not been more than about ½ magnitude. I have not observed this star so long as she has; but so far as my observations go they tend to show that the period, if regular, is about 1 year, which would account for the slightness of variation observed by her in spring months only, though even at the periods she mentions I have observed a somewhat greater range than she gives.

West Hendon House, Sunderland,
1898, Dec. 23.

Yours faithfully,
T. W. BACKHOUSE.

Sunset from a Legal Point of View.

GENTLEMEN,—

As a Magistrate of very many years' experience I am sometimes tempted to concur with the late Mr. Bumble in his dictum that "the law is a hass"; but I can hardly conceive of anyone on the Bench so far stultifying it—and himself—as to fine a cyclist for not having his lamp lighted while yet the Sun is visible.

By the 43 and 44 Vict. c. 9, it is enacted that whenever any expression of time occurs in any Act of Parliament, &c., such time shall (unless it is otherwise specifically stated) be, in Great Britain, Greenwich Mean Time. Just so; and a man at Dumbarton would not be allowed to plead that something which happened there at Greenwich Mean Noon in reality occurred at $11^h\ 41^m\ 44^s$ A.M., just because his longitude happened to be $18^m\ 16^s$ West of the Prime Meridian. But the operation in fact and effect of this would be that the instant of disappearance of the Sun below the horizon at Dumbarton must be reckoned in Greenwich Mean Time and not in Local Mean Time. The setting of the Sun is an objective fact, as much so as the firing of a gun or the fall of a meteor, and may even be accelerated or retarded by the condition of the atmosphere.

Forest Lodge, Maresfield, Uckfield,
1899, Jan. 9.

Yours faithfully,
WILLIAM NOBLE.

[Capt. Noble does not seem to have realized that the point of the letter in our last number was as to whether "sunset" in the "eye of the law" means in all cases sunset at Greenwich, as Mr. Chambers suggested in our number last August, or whether it means sunset at the place where the alleged offence happens. —Eds.]

As the Editors have kindly sent me a proof of the above letter for correction, I have thus seen their comment upon it. My own contention, then, is that "sunset" at any *given station* legally means the instant, in Greenwich Mean Time, at which, as the Sun sinks, his upper limb becomes tangent to the horizon *at that station.*—W. N.

On the Discovery of a certain Proper Motion.

Gentlemen,—

With regard to the two notices which have appeared in 'The Observatory' for February and November last, relative to the remarkable star C.Z. 5^{h} 243, allow me to say a few words.

The notice of the discovery of the extraordinary proper motion of this star was originally inserted in the 'Astronomische Nachrichten,' No. 3466, by Professor Kapteyn, and he there clearly and fairly states that the discovery emanated from the C.P.D. and the work of Mr. Innes and himself.

In the 'Observatory' for February last, referring to Kapteyn's note above mentioned, you attribute the discovery *entirely* to Professor Kapteyn, and others appear to have followed your lead.

In your subsequent note ('Observatory,' Nov. 1898, p. 409) you attribute the *kudos* entirely to Mr. Innes, as a logical sequence of the short history of the discovery given in my Annual Report for 1897.

Allow me to point out that neither one nor the other of these two latter conclusions is a true or fair one.

Professor Kapteyn's original statement is sufficiently explicit, viz. :—"Die Entdeckung ist aus den Arbeiten für die C.P.D. hervorgegangen, also aus den Arbeiten des Cap-Astronomen (Innes) und meinen eigenen."

The more detailed account of the matter given in my Report is as follows; and as many of your readers may not otherwise have access to it, I transcribe the passage :—

> By far the most interesting result of this examination has been the discovery of an 8th magnitude star having an annual proper motion of nearly 9″ in arc, *the largest proper motion yet known.*
>
> In regard to this star (No. 8 of List I.) Kapteyn had remarked :—
>
> No. 8. 5^{h} 6^{m} $40^{s}\cdot6$ -44° $58''\cdot2$; C.Z. V. 243. May 8. "Certainly missing on two plates."
>
> On 1897, Feb. 2, Mr. Innes looked for this star, and found none in the given position; but he found a full yellow star, 8·3 mag., about 15 sec. in R.A. from the required place. The following day I transmitted to Prof. Kapteyn this note of Mr. Innes, with his query "Do you find the star 5^{h} 6^{m} $56^{s}\cdot0$ -44° $58'$ on the C.P.D. plates? It is difficult to think the C.Z. wrong 15^{s} on two occasions, or that the bright star 5^{h} 6^{m} 56^{s} could have been overlooked. Can this be proper motion?"
>
> On August 24 Kapteyn wrote requesting that the star in question should be again looked up, because the observations now stood thus :—

		Mag.	α. h m s	° ′	
Gould Z.C.......	1873·0	8	5 6 40·6	−44 58·2	2 obs.
C.P.D.	1890·1	9·2	6 50·8	59·5	4 obs.
Innes	1897·0	8·3	6 56·0	58·0	

the R.A. agreeing very well on the supposition of a proper motion of $0^s{\cdot}64$, but not so the Declinations. Could there be a mistake in Mr. Innes' Declination? On September 22nd I replied that there was no doubt that the star C.Z. V. 243 is the star of greatest known proper motion; that Innes' observation of 1897·0 had not been an observation in Declination, because, finding no star in the required R.A., he had simply noted the neighbouring star, differing 15^s in R.A., and having *about* the same Declination as the missing star. But on Feb. 15 he had re-observed the star, and found $\alpha\ 5^h\ 6^m\ 56^s{\cdot}0$, $\delta\ -45^\circ\ 0'{\cdot}4$, and that the estimated position of the star on one of the Catalogue plates made a proper motion of about $+0^s{\cdot}64$ and $-0''{\cdot}1$ in Declination certain. On October 27 I forwarded results of two meridian observations of the star made on October 23 and 24. A preliminary note has been communicated to the Astron. Nach. 3466, by Prof. Kapteyn. A complete investigation of the proper motion and parallax of this remarkable object will be made at the Cape.

Thus no fair-minded reader can say that the discovery of the large proper motion of this star was due more to one observer than the other; certainly nothing was further from my intention than to imply any such conclusion. Questions of this kind may often be created in regard to many other discoveries connected with the C.P.D. work.

In many cases the C.P.D. plates and other existing data have been sufficient to enable Kapteyn to pronounce decidedly on the variability or proper motions of certain stars. In other cases it was necessary to re-observe the sky, or re-photograph the region, and only patient observation and comparison (in some cases for many months) could establish whether certain stars are variable or not, or whether apparent proper motions were real, or due to errors in previous catalogues, &c. All concerned in the photographic work of the C.P.D. are perfectly satisfied that they are co-operating in a useful work, and that the fullest credit for the share of each in that work has been and always will be given.

Royal Observatory,
Cape of Good Hope,
1898, Dec. 2.

Yours faithfully,
DAVID GILL.

[Though the matter it relates to is of small importance, we have given considerable space to this letter from Dr. Gill, because, in the first place, it accuses us of some want of judgment, and secondly because it raises a nice point in the ethics of the profession as to which we should like to say a few words.

In our note of last February we said "Dr. Kapteyn publishes a note stating that in examining a plate he discovered a star...." These are facts, except that we should rather have said "discovered the omission of a star...." We proceeded to imply that the work for deduction of the P.M. was done at the Cape. This was copied into other publications, and as Mr. Innes might feel aggrieved, when opportunity offered, that is when Dr. Gill gave the facts, we repeated these facts, mentioning Mr. Innes by name, and used the word *kudos*, thinking it less emphatic than *honour*.

Now all this seems very trivial and very hair-splitting, but it is not without its reasons. The suggestion of a possible proper motion occurs, perhaps, once a day in a hard-working observatory; the verification of such is rarer. This is mentioned to explain that no one has shown unusual genius; but because this is the largest proper motion hitherto known, some name will be attached to it in sensational astronomical articles, and the owner of that name will acquire some notoriety. We should fancy the staff of the Cape Observatory will feel aggrieved if that name is Kapteyn, although Dr. Gill does say they are satisfied with the credit given them for their share in the work. The trouble seems to have arisen because Dr. Kapteyn was allowed to publish, in his own name, results of work which emanates from the Cape Observatory. It always seems to us that such results should be published as coming from the Observatory, and if the Director of the Observatory thinks fit to award especial praise to any of his staff, he can generally find opportunity to do so; but we think that no member of the staff of an Observatory should, in justice to his colleagues, make a spurious reputation from circumstances more or less accidental. These are our views, and we think the wording of our notes is consistent with them.—Eds.]

NOTES.

Comet Notes.—Some further observations of Chase's Comet have been obtained, but it is a very faint object, and only visible in large instruments. An ephemeris for February was given last month.

The 'Astrophysical Journal' for December contains a description of some photographs of Comet *i* 1898 (Brooks), obtained with the Crossley 3-foot reflector at the Lick Observatory by Prof. Keeler.

The best photograph was obtained on Nov. 5, when a narrow straight tail, 23′ long, was shown, resembling that of Comet *b* 1894 (Gale). A photograph with the Willard lens showed a tail 45′ long.

The tail was not exactly opposite the Sun. On Nov. 5 it made an angle of 5° with the radius vector produced, and on Nov. 12 an angle of 2° in the other direction. The photograph of Nov. 10 showed that the head was composed of two clearly-separated nebulous masses surrounded by the nearly circular coma. This is supposed to be a real division and not merely a photographic defect, although visual observations showed no trace of it. The photographs of the following day showed no trace of this division, so if real it was very transitory.

Ast. Nach. 3537 contains a note on the determination of the orbit of Comet 1847 V. (Brorsen) by A. Schobloch. He makes the period 80·75 years, which is in good agreement with that found by Gould; he proposes to investigate the limits within which the period must lie.

Ast. Journ. 451 contains the following orbit of Comet 1895 II. (Swift) by H. R. Morgan. The orbit does not represent the observations so well as could be wished, the residuals of the later observations being all in the same direction:—

Epoch	1895 Aug. 24·5 G.M.T.	
M	0° 30′ 10″·8	
ω	167 46 10 ·0	1895·0
☊	170 18 25 ·0	
i	3 0 26 ·7	
ϕ	40 43 47·2	
μ	491″·509	
log a	0·572317	
Period	7^y·220	

A. C. D. C.

Minor Planet Notes.—The planet 433 (DQ) has been named Eros. Thanks to the zeal of Prof. Chandler[*] in computing accurate ephemerides, and the patience and perseverance of Prof. Pickering and Mrs. Fleming in examining the immense stock of photographs taken at Harvard and Arequipa, the planet has been found on four plates taken in 1896, and on twelve plates taken in 1893–94 (Ast. Journ. 452). The following orbit was deduced by Prof. Chandler from the oppositions of 1896, 1898, which satisfies the observations of 1894, with no error exceeding 8^s in R.A., or 1′ in Dec., so that it is probably very near the truth:—

Epoch	1898 Aug. 31·5 G.M.T.	
M	221° 35′ 45″·6	
ω	177 37 56 ·0	1898·0.
☊	303 31 57 ·1	
π	121 9 53 ·1	
i	10 50 11 ·8	
ϕ	12 52 9 ·8	
μ	2015″·2326	
log a......	0·1637876	
Period	643^d·10	

On 1894 Jan. 23 the planet's distance from the Earth was 0·153, so that this was the most favourable opposition possible. It was then of the seventh magnitude, being brighter than the ninth magnitude from 1893 Nov. 20 to 1894 Mar. 21.

Prof. Chandler proposes to definitively determine the orbit, using the results of all three oppositions and applying perturbations.

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[*] See p. 93.

We give Prof. Chandler's ephemeris for 1893–94 (abridged), as it is quite possible that other photographs of the planet may be found now that its accurate position is known. It is for Greenwich midnight:—

	R.A.		N. Dec.		R.A.		Dec.
	h	m	°		h	m	°
1893 Oct. 27	5	56·4	53·3	1894 Jan. 31......	7	23·6	14·3 N.
Nov. 8	6	31·7	55·8	Feb. 12......	7	27·6	2·6 N.
20	7	4·3	57·4	24......	7	39·3	4·9 S.
Dec. 2	7	30·5	57·7	Mar. 8......	7	56·7	9·2
14	7	45·3	56·1	20......	8	18·5	11·6
26	7	46·7	51·6	Apr. 1......	8	43·4	12·8
1894 Jan. 7	7	38·0	42·6	13......	9	10·6	13·5
19	7	27·8	29·0	25......	9	39·0	14·0 S.

If plotted on a map it will be seen that the motion in longitude is *direct* throughout, though in Right Ascension there is a small retrogradation.

The following ephemeris for the next opposition has also been collected fron the new elements. This is also for Greenwich midnight:—

	R.A.			N. Dec.		Distance.	Magnitude.
	h	m	s	°	′		
1900 Nov. 10....	1	56	24	54	23	0·3747	9·38
18....	1	41	8	53	37	0·3533	
26....	1	30	40	51	52	0·3383	9·07
Dec. 4....	1	26	32	49	26	0·3268	
12....	1	29	22	46	23	0·3200	8·88
20....	1	38	48	43	5	0·3162	
28....	1	54	28	39	39	0·3154	8·78
1901 Jan. 5....	2	14	34	36	12	0·3176	
13....	2	38	6	32	45	0·3219	8·78

In computing the magnitude no account is taken of loss of light through phase. This will, however, be very considerable, for at quadrature near perihelion only three-quarters of the apparent disc is illuminated. Special precautions would seem to be required to avoid systematic errors in parallax measures arising from the unsymmetrical illumination of the disc. The solar parallax is already known with such accuracy that the smallest source of possible systematic error should be eliminated in any attempt to obtain an improved value.

The planet Eros is still under observation, and is likely to be followed for some time longer; the following ephemeris, for Paris midnight, is by M. Fayet (Ast. Nach. 3538):—

	R.A.			N. Dec.			R.A.			N. Dec	
1899.	h	m	s	°	′	1899.	h	m	s	°	′
Feb. 1....	0	12	52	10	50	Feb. 17....	0	59	27	14	41
5....	0	24	8	11	47	21....	1	11	46	15	38
9....	0	35	39	12	46	25....	1	24	22	16	33
13....	0	47	25	13	43	Mar. 1....	1	37	15	17	28

A. C. D. C.

THE MASS OF MERCURY.—Dr. Hill, in the 'Astronomical Journal,' No. 452, has attempted to estimate the mass of Mercury by the hypothesis that its circumstances are not very different from those of Venus, the Earth, and the Moon, when due allowance is made for the fact that as Mercury is smaller than the other planets, the pressure, and therefore the density, in its interior is likely to be less. The method is only of value in consideration of the extreme uncertainty in the mass of Mercury, various estimates ranging from a three-millionth to a twelve-millionth part of the mass of the Sun. It is, perhaps, not surprising that, with a rough knowledge of the mass in question to guide his hypotheses, Dr. Hill has arrived at a result more or less in accordance with the best determinations by other methods; but the paper is interesting, as tending to confirm the older results as somewhere near the truth. P. H. C.

THE BEGINNING OF THE TWENTIETH CENTURY.—The discussion about the exact date when the next century will begin has been occupying some space in periodical literature during the past month. The Astronomer Royal granted an interview to the 'Daily Telegraph' *à propos* of the subject, which 'Punch' referred to in burlesque. In looking up the literature on the point we came across a pamphlet, printed for private circulation in 1870, entitled 'The Century Dinner.' Two gentlemen, Mr. J. C. Bolton and Mr. Walter Paterson, who seemed to have been Scotch lawyers of some eminence, made a bet as to which century the year 1800 belonged. They referred the matter to the Lord Dean of Guild as Arbiter, and gave a dinner at Glasgow, where evidence was taken in a merry spirit on this important question, of which this pamphlet is an account. The decision given was, as most persons would expect, that the year 1800 belonged to the 18th century, the bases of the decision being chiefly the dictum contained in Sir John Herschel's 'Astronomy,' that there was no year 0, but that the year A.D. 1 immediately followed the year B.C. 1, and the opinion of astronomical writers, who state that the planet Ceres, discovered 1801, Jan. 1, was found on the first day of the 19th century. The other side urged that the 'Times' of 1849, Dec. 31, speaks of the "close of the first half of the 19th century"; but it was added that, a few days later, after some public discussion, the opinion thus implied was withdrawn. Dean Stanley was also quoted as being on the losing side, because in one of his works he writes, "the year 1700, the first year of the 18th century"; but in answer to a letter he acknowledged that he had no deliberate opinion on the subject. The Astronomer Royal, by the hand of Mr. Stone, gave evidence for the winner, so it may be taken that the question has been thoroughly considered, and the humorous little book can be accepted as authority that the next century does not begin until January 1st, 1901. We read in it also that several papers on the subject appeared in Germany in the years 1799 and 1800, and have formed the subject of a very amusing little play by Kotzebue.

We have received a letter, on the subject of Messrs. Cooke & Son's dome, from Prof. E. Becker, Director of the Strasburg Observatory, who points out that a dome made on this plan has been in existence at Strasburg for some years. The construction of the altazimuth of that observatory necessitated a very wide opening, and the plan of a shutterless dome met the difficulty of the case, as the sliding of the two halves is sufficient to give a breadth of 2½ metres. Prof. Winnecke, the former Director of the Strasburg Observatory, briefly described this at a meeting of the Astronomische Gesellschaft in 1879. We can only say that though the design had been thus anticipated, we are sure that the conception of the transit-pavilion dome at Greenwich was an original idea, and that this is merely another instance of a well-worn maxim.

We hear that Mr. R. A. Gregory, whose name will be remembered in connection with our contemporary 'Nature,' has been appointed Professor of Astronomy in Queen's College, London, in succession to Dr. J. D. McClure. This college, which has a Royal Charter, celebrated its Jubilee last year.

It is announced in 'Nature' that Prof. F. Küstner, the Director of the Bonn Observatory, has been appointed to take over the Directorship of the Hamburg Observatory, in succession to Prof. G. Rümker, who has, through prolonged illness, been compelled to retire.

As closing the correspondence lately published on the "legal time of sunset," it may be put on record that, on January 26 last, two of Her Majesty's judges ruled that the word "sunset," as used in the Act relating to the lighting of bicycle lamps, means the time when the Sun sets at the place where the bicyclist then is, and does not mean the time of sunset at Greenwich, as had been held by the magistrates.

Messrs. Ginn & Co., of New York, inform us that Miss Mary E. Byrd, Director of the Smith College Observatory, is preparing a Manual in Astronomy. We look forward with pleasant anticipation to seeing the work of this gifted lady, who, some years ago, wrote a remarkably bright article for this magazine.

It is announced (Ast. Nach. 3539) that Padre Angel Rodriguez de Prada is now the Director of the Vatican Observatory in Rome. It appears from this that the announcement made about twelve months ago that the Pope had appointed Prof. Searle to the office was incorrect, or that the appointment was not taken up.

We regret to see the announcement of the death, which took place on December 29 last, of Prof. E. Abelmann, of Wilna. It will be remembered that important papers on the meteor-showers of last November by this gentleman were published in our number for that month.

FROM AN OXFORD NOTE-BOOK.

THE completion of a long and laborious but very successful piece of work must be a wonderful satisfaction to its author, and our best congratulations are due, and are heartily given, to Professor Hermann Struve, whose completed memoir * on Saturn's Satellites has now been published. The observations with the powerful 30-inch equatorial of the Poulkova Observatory (in sequel to others with the 15-inch) were made in the years 1886 to 1892, consisting in great part of micrometrical comparisons of one satellite, not with the ball or ring of Saturn, but with another satellite. This method of observation, while it added to the mathematical difficulties of the discussion, avoided various systematic errors to which other methods are liable; and it soon became clear, not only that the observations were very good, but that new facts about the movements of the orbits of the satellites were going to come out of them. Indications of these results were given in some preliminary discussions in the Ast. Nach. 2945–6 and 2983–4.

THE preliminary discussions above mentioned were noticed in the Annual Reports of the Council of the R. A. S. for 1890 and 1891. The initials attached to the notices are well-known ones, J. C. A. and A. M. Such initials are not attached to throw responsibility on the authors rather than on the Council; for the Report and all that it contains the Council alone is responsible, sometimes exercising a severe censorship on what has been written by unfortunate secretaries and others in order that nothing may be published which has not their entire approval. But there is no need to absolutely shut our eyes when we see certain well-known initials printed, and I should like here to mention a little incident connected with the first notice which, at this distance of time, there can be scarcely an impropriety in mentioning. If anyone will take the trouble to read the notice, he will, I think, be struck by the fact that, while it gives a faithful and clear account of the work, scarcely a single adjective is used to express anything like admiration for the beautiful results obtained. I was so much struck with this fact at the time that I ventured to ask the late Professor Adams whether he did not think it a fine piece of work. He replied that he had been so much astonished at the smallness of the residuals that he could scarcely bring himself to believe them! And thus his characteristic caution had led to his writing the perfectly impartial account which is to be read in Mon. Not. vol. l. p. 222. Stronger testimony to the excellence of the observations and the discussion could scarcely be obtained.

THE second notice, by the late Albert Marth (vol. li. p. 251) also

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* Publications de l'Observatoire Central Nicolas, Série II. Vol. XI. 'Beobachtungen der Saturnstrabanten am 30-zölligen Pulkowaer Refractor, von Hermann Struve, Professor an der Universität Königsberg.' St.-Pétersbourg, 1898.

has a touch characteristic of its author. Prof. H. Struve had discussed, along with his own, the older observations of Mimas, of which he gave a list (Ast. Nach. 2983, p. 106). Mr. Marth was promptly down on him for incompleteness :—"The scattered measurements and estimated positions of *Mimas*, made by Bond 1848–49 and 1852, and published in 1857 in vol. ii. of the 'Annals of the Observatory of Harvard College,' seem to have been overlooked, and will furnish some additional serviceable evidence" (M. N. vol. li. p. 252). In the complete discussion now published these observations are included, though they do not seem to add much "serviceable evidence."

THIS is the season when many of us are reviewing the work of the last year for one purpose or another: writing notes to be pulled to pieces by the ferocious Council of the R.A.S., or stocktaking in our observatories, or compiling reviews for daily papers and magazines. There was a curiously constructed sentence in the 'Times' review of Astronomy in 1898. I alter the names and dates a little for obvious reasons; but it ran like this :—

> . . . . . and with Mr. John Jones, who died prematurely on August 15 last, of the 'Astronomical Directory,' a useful little work of reference.

The author will not forgive me from taking it out of its context, which is indeed scarcely fair; but I trust his anonymity is sufficiently profound to protect him.

THE Christmas lectures delivered by Sir R. S. Ball seem to have been a greater success than ever. He may be interested to know that they have brought other astronomers a fine crop of queries to answer; but, after all, there is very little to grumble at in this. One correspondent kindly sends me a cutting from 'The Outlook' of Jan. 21, dealing with the discovery of Eros, or, as it is therein designated, the "New Minor Planet 433, D. 2." The difference between the new astronomy and the old is put into a very small nutshell as follows :—

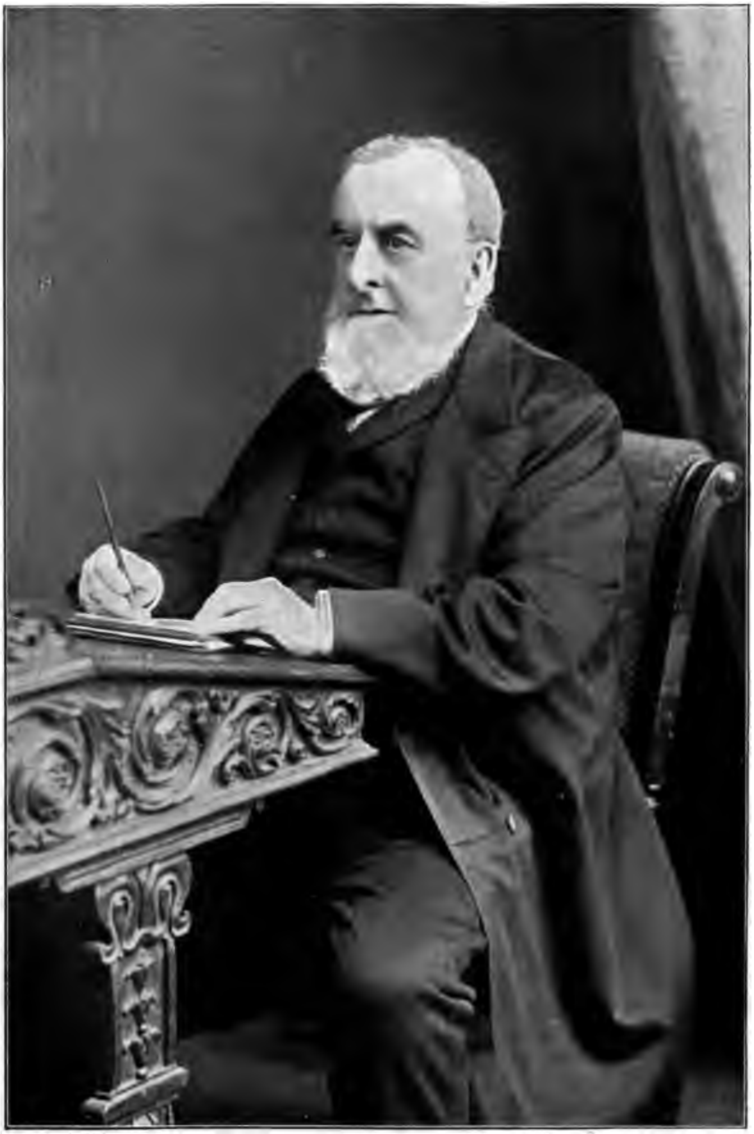
> In days of old the luckless astronomer had to sit with his eye glued to his telescope all night long. Nowadays he simply puts in place of his eye a series of photographic plates or films, which are moved across the eyepiece of the telescope in regular succession by a chronographic mechanism, goes peacefully off to bed, and in the morning comes back to see what he has caught in his star-trap.

Now this is more or less true, though rather less than more; but I hope Sir Robert will not impress this fact *too* strongly on the public. Otherwise we shall lose a good deal in the way of sentimental regard. It has often given me great pleasure, and I am sure others must have shared the experience, to find that the lucklessness of "having to sit eye-glued all night long" invested the astronomer with considerable interest in the minds of charming people, let us say beautiful young ladies who do not know anything about astronomy. Of course, being honest men, we naturally do our best to set them right in their views; but we like to do this gently and rather slowly. I would beg Sir Robert not to tell every one in this direct fashion of the change in the state of affairs, especially as it is not altogether true; for *some one* has to put in new plates and take out old ones.

We again learn a valuable lesson as to the proper uses of photography from Professor Pickering, of Harvard. Thanks to his persistent photography of the whole sky the planet Eros has been found on more than a dozen plates taken at the oppositions of 1894 and 1896. How long will it take us to realize that the sky is not to be photographed once and then the work considered done? There are probably few astronomers who would not disavow this notion individually, and yet it enters essentially into many of the resolutions of the Committee on the Astrographic Chart. We want, not *one* survey taking a lifetime, but a series of surveys at short intervals: five or six years if you like, but better five or six months, and better still five or six days. Professor Pickering is doing this, and hence the wealth of discovery that falls to his share. We at Oxford were sorry not to find just one plate with the planet on it in 1894; we were very near it; on one night we had two plates just on opposite sides of the proper region, but nothing between. But it would have been good luck if we had got it; Professor Pickering has got it by good management and well deserves his success. I would also add my respectful congratulations to Mrs. Fleming. She must have worked very hard.

It is after all just as well that the new planet 433 has been named Eros instead of gloomy Pluto. I have not seen any account of the reasons for the name: perhaps there are none capable of being stated. But it is worthy of note that such people as found amusement in making allusions to the tender passion on the occasions of the Transit of Venus will not be robbed of their occupation when the solar parallax is in danger from the new planet. Indeed there is a good deal more meaning in the "Opposition of Eros" than in the old phrase.

Is it known to many that there is in existence an operetta called the 'Transit of Venus'? The scene is laid at an observatory on the Glockenhorn, near the watering-place of Bad Eggenstein, whither a number of students come to observe the Transit; and there is one exciting moment when a gentleman, trying to escape from the gendarmes, runs up the observing-chair and jumps into the telescope, smashing the object-glass as though it were paper, and coming out at the eye-end to take refuge in the sidereal clock. I gave some friends a description of the play the other day, and they seemed not to believe me; but it is an actual play by J. T. Tanner and Adrian Ross, and was played at Oxford not so very long ago, to my great delight. It had never occurred to me before what a good scene for a pantomime could be made out of an observatory, and I hope it will not occur to any one else too seriously.



EDWIN DUNKIN, F.R.S.

THE OBSERVATORY,

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ANNIVERSARY MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1899 February 10.

Sir R. S. BALL, LL.D., F.R.S., *President*, in the Chair.

Secretaries: Prof. H. H. TURNER, M.A., B.Sc., F.R.S., and H. F. NEWALL, M.A.

Mr. Newall read the Minutes of the last Anniversay Meeting, which were confirmed.

Mr. Inwards read the Report of the Auditors, which stated that they had examined the Treasurer's accounts for the year 1898, and certified the same to be correct. The cash in hand on 1898 December 31 was £565 6*s*. 6*d*., as compared with £244 11*s*. 2*d*. on 1898 January 1. They had inspected the books and other property of the Society, which appeared to them to be in good condition.

Prof. Turner read extracts from some of the obituary notices of Fellows and Associates of the Society who had died during the year, of which the following is the list:—J. G. Barclay, L. H. Bradford, Latimer Clark, Edwin Dunkin, John Hippisley, W. B. Hutchinson, Henry Perigal, Rev. T. J. Potter, Rev. Bartholomew Price, Herbert Sadler, A. F. Smith, J. E. de Villiers, George Williams, Rev. A. Wrigley, who were Fellows; and Cyrille Souillart, Associate. Photographs of Mr. Perigal were thrown on the screen.

Mr. Newall read extracts from the Reports of Observatories for the past year; diagrams in illustration of these were exhibited, which showed the progress of the work of the Astrographic Chart at Greenwich and at Oxford, the rate of work with the transit-circle at Greenwich, subjects of observation with the 28-inch equatorial at Greenwich, and the new Cambridge telescope.

Prof. Turner gave the heads of, and read extracts from the notes on the progress of Astronomy during the past year which were included in the Report of the Council. The subjects of these notes are:—Discovery of Minor Planets and Comets, Meteoric Astronomy, the Physical Aspect of the Planets, Lunar Photography, Star Catalogues, Sun-spots and Faculæ, Double Stars, Poincaré's 'Mécanique Céleste,' the Astrographic Chart, and Spectroscopy.

The President then announced that the Gold Medal of the Society had been awarded by the Council to Mr. Frank McClean for his splendid series of photographs of the spectra of all stars of magnitude 3½ and brighter in both hemispheres. Having described Mr. McClean's work in some detail, and referred to his magnificent benefactions to the science, the President, in an eloquent peroration, pointed out that the Medal was in no way awarded because of the benefactions but solely in recognition of Mr. McClean's skill and zeal in making this large and valuable series of spectrographs with his own unaided hands.

Lord Rosse proposed, and *Mr. Lynn* seconded, that the Report now read be received and adopted, and that it be printed and circulated in the usual manner with the Report of the Auditors and the President's Address.

The President appointed Mr. Crommelin and Mr. Kempthorne Scrutineers of the Ballot.

The Ballot was then proceeded with, and the following gentlemen were elected to serve as Officers of the Society for the ensuing year:—

President: Prof. G. H. Darwin. *Vice-Presidents*: Capt. W. de W. Abney, Sir R. S. Ball, W. H. M. Christie (Astronomer Royal), Dr. J. W. L. Glaisher. *Treasurer*: E. B. Knobel. *Secretaries*: F. W. Dyson, H. F. Newall. *Foreign Secretary*: Sir William Huggins. *Council*: Dr. A. A. Common, Dr. A. M. W. Downing, John Evershed, Capt. E. H. Hills, Mr. Frank McClean, Major P. A. MacMahon, W. H. Maw, Capt. William Noble, Dr. A. A. Rambaut, G. M. Seabroke, W. G. Thackeray, Prof. H. H. Turner.

Admiral Sir E. Ommanney proposed, and *Mr. Inwards* seconded, that the thanks of the Meeting be given to the President, Vice-President, and Secretaries who now retire, for their services in promoting the objects of the Society.

On the motion of *Prof. A. Herschel*, seconded by *Mr. Cushing*, a vote of thanks was accorded the Auditors of the Treasurer's accounts and to the Scrutineers of the Ballot.

The following gentlemen were elected Fellows of the Society:—

Mons. E. M. Antoniadi, Observatory, Juvisy, Seine-et-Oise, France.

John Jepson Atkinson, Cosgrove Priory, Stony Stratford.

Dr. W. Lee Dickinson, 9 Chesterfield Street, Mayfair, W.

John James Hall, Observatory Cottage, Datchet Road, Slough.

Joseph Larmor, M.A., Sc.D., F.R.S., St. John's College, Cambridge.

John H. Reynolds, Malvern House, Trinity Road, Birchfield, Birmingham.

Charles Almeric Rumsey, B.A., Dulwich College, S.W.

Charles Stevens, 10 Wemyss Road, Blackheath, S.E.

William Harold Tingey, B.A., F.R.Met.Soc., Rede Court, Rochester, Kent.

Thomas Weir, 56 Parkfield Street, Moss Lane East, Manchester.

Algernon Charles Legge Wilkinson, B.A., Trinity College, Cambridge.

The following Candidates were proposed for election as Fellows of the Society:—

Ernest William Barnes, B.A., Fellow of Trinity College, Cambridge (proposed by E. T. Whittaker).

Samuel Chatwood, Engineer, &c., Broad Oak Park, Worsley, near Manchester (proposed by A. H. Fison).

Rev. W. B. K. Francis, Chaplain and Naval Instructor, R.N., H.M.S. 'Boscawen,' Portland (proposed by W. J. S. Lockyer).

Windeyer George Lingham, Master Mariner, 1 Caldervale Road, Clapham, S.W. (proposed by W. F. Gale).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The third ordinary Meeting of the ninth Session of the Association was held at Sion College on Wednesday, January 25, *Mr. W. H. Maw*, President, in the Chair.

The President announced that Mr. G. T. Davis, one of the original founders of the Association, had presented them with a 3-inch refracting telescope and tripod stand. The hearty thanks of the Meeting were voted to Mr. Davis for his gift.

Mr. G. J. Newbegin exhibited, by the aid of the lantern, fifteen photographs of sun-spots in completion of a series from September 10 to December 9, the negatives of which he handed over to the Association.

The President said they were all grateful to Mr. Newbegin for the series of admirable photographs which he had from time to time brought before them.

The President then introduced Prof. C. V. Boys, who had promised to give an address on "The Determination of the Newtonian Constant of Gravitation."

Prof. Boys commenced by referring to Kepler's discovery of the three laws of planetary motion. Sir Isaac Newton had demonstrated that a single principle—namely, that every particle in the universe attracts every other particle towards itself with a force which is proportional to the product of their masses divided by the square of the distance between them—would completely account for the three laws that Kepler had given to the world. The merit of Newton's discovery lay, not in the suggestion that a single principle would account for Kepler's laws, but in his supplying mathematical proof that it did so. But though Newton's great discovery enabled us to compare the masses of the Sun, the planets, and their satellites, it yet left us still ignorant how many tons of matter went to make any one of them. In order to solve this problem we might either make observations on the disturbance of the Earth's gravitation by the action of isolated parts of it, such as the relative attraction of an isolated mountain, or we might make an artificial planet of our own and find the attraction which it exerted. The Newtonian Constant would be known if we knew the force of attraction between two bodies which we could completely measure and weigh. On the C.G.S. system of measurement the Newtonian Constant is equal to the force of attraction in dynes between two balls weighing a gramme each and having their centres one centimètre apart. Newton's equation runs:

$$\text{Force} = G\,\frac{\text{Mass} \times \text{Mass}}{\text{Distance}^2}.$$

If all the other quantities are known, no matter in what units they are measured, G is known. So long as there are two unknowns in one equation we cannot obtain a complete solution. After referring to the experiments of Maskelyne and others, Prof. Boys proceeded to describe his own investigations with an artificial planet. The amount of attraction that would be exercised by any practicable mass of metal would be so minute, compared with the attraction of the Earth, that the deviation of a plumb-line from the true vertical could not be accurately ascertained. The invention of the torsion-balance by the Rev. John Mitchell gave the means for detecting a very small displacement.

Prof. Boys then described his quartz fibres, which, though strong as steel, were infinitely delicate and might be produced of any imaginable degree of fineness without losing their elasticity. This enabled him to reduce all the dimensions to an extent which caused the forces, and especially the couples, to be insignificant in comparison with any which had been within the reach of previous experimenters. Cavendish had a force equal to $\frac{1}{3650}$ of a grain weight to measure on an arm 1 inch long; he (Prof. Boys) had less than a five-millionth on an arm of that length. One advantage gained by the use of small apparatus, in which alone the attracting balls could be made large compared with the length of the beam, is the increased sensibility, the greater angle of deflection produced by the attractions when the period of oscillation was the same. With the aid of a series of lantern-slide views, Prof. Boys described in detail the apparatus he had constructed in the vaults under the Clarendon Laboratory at Oxford, and the precautions taken to avoid variations of temperature, air-currents, and vibrations from trains and passing traffic, alluding in passing to a serious disturbance to his observations caused by an earthquake which took place in Roumania. In conclusion, Prof. Boys stated that the result of his observations was that "the force with which two spheres weighing a gramme each, with their centres one centimètre apart, attract one another is 6·6576 divided by $(10)^8$ dynes, and that the mean density of the Earth is 5·5270 times that of water." In other words the total weight of the Earth was 5,882,064,000,000,000,000,000 tons.

Since he had completed his experiments, Dr. C. Braun, at Mariaschein, in Bohemia, had carried out the same research, but in a somewhat different manner. His apparatus was rather larger, and he used wires instead of quartz fibres; but the experiments had been conducted in a Sprengel vacuum, and had undoubtedly been carried out with the greatest skill and care. Father Braun's result for the mean density of the Earth from his observations was 5·5273, agreeing precisely with his own to the first four figures. He hoped to send Father Braun a pair of lead balls and some quartz fibres, and so, by combining the special advantages of the two methods, to secure a still more accurate determination.

The President said he was sure all present would agree that they had had a great treat in listening to the address of Prof. Boys. Personally, he did not know which to admire most—the admirable accuracy of the results which Prof. Boys had obtained, or the great ingenuity of the apparatus by which he had obtained them.

Capt. Noble said when a man arrived at his time of life he had heard probably a good many lectures; he personally had, and he thought that the two most interesting to which he had ever listened were, one by Prof. Ramsay on Argon, and the one that had been delivered that night by Prof. Boys upon the wonderful results at which he had arrived. If they came to consider the minuteness of the measures and the delicacy of the apparatus employed, it seemed almost incredible that the thing could have been done.

Mr. Crommelin wished to ask Prof. Boys whether he was troubled at all in his measurements by electrical action between the different spheres. He did not know whether they were insulated from one another, but he thought it possible that the electrification of the spheres might have introduced some attraction which would be comparable to the gravitation attraction between them.

Prof. Boys, in reply, said that he had not overlooked the question of the possibility of electric and magnetic disturbances and had taken every precaution to avoid them. The small attracted balls were of pure gold, and the centre tube was polished and electro-plated with pure gold. Then the tube in which the balls moved was concentric with their axis of rotation, so in their movement they did not approach or recede from its walls. Further, as in several experiments the gold balls were all sorts of sizes and the high and low ones changed positions, any disturbances, if they had existed, would have shown as irregularities, but the uniformity of the results was evidence that they did not occur to a more than infinitesimal extent. To test it for magnetic effect he had mounted a bar magnet near the instrument and caused it to move. This must have produced a magnetic force many thousand times that, if any, exerted by impurities in the leaden balls, but no effect on the delicate suspension could be detected, so that he was sure that magnetism could not produce any disturbance.

After some remarks from the President, the meeting adjourned.

ROYAL METEOROLOGICAL SOCIETY.

The Monthly Meeting of this Society was held on Wednesday evening, February 15, at the Institution of Civil Engineers, *Mr. F. C. Bayard, LL.M.*, President, in the Chair.

Mr. E. Mawley read his Annual Report on the Phenological Observations, and stated that the weather of the past year, taken as a whole, had been throughout the British Isles very warm and dry. Wild plants blossomed much in advance of their average dates until about the end of March, but after that time until the close of the flowering-season they were mostly late in coming into bloom. Favoured by the rains in May, the crop of hay was everywhere a remarkably heavy one, but the long drought which followed dried up the pastures and caused a scanty yield of roots. The dry season suited the cereals admirably, and especially the wheat, of which there was a very abundant crop. The yield of barley was nearly as exceptional, while that of oats, except in the north-east of England and in Scotland, was also unusually good. There was a splendid crop of potatoes in Ireland and in parts of Scotland, but elsewhere the yield was, on the whole, moderate. Apples, pears, and plums flowered abundantly, but adverse weather conditions, and the dry subsoil in the spring, caused an irregular "set" of fruit, so that in all parts of the kingdom these crops were, as a rule, below average. On the other hand, there were good crops of all the smaller fruits.

A paper by *Prof. W. M. Davis*, of Harvard University, U.S., on "The Circulation of the Atmosphere," was read by the Secretary. The author said that, although the circulation of the atmosphere is one of the earliest and one of the latest problems of meteorology, its treatment is ordinarily inadequate, inasmuch as the serious student seldom gains from the text-books in current use a comprehensive view of the great problem. After giving a brief historical development of the subject, the author went more particularly into the question of the outflowing polar winds, especially in the Antarctic regions. He called attention to the remarks made by Dr. Buchan, at the conference on the Scientific Advantages of an Antarctic Expedition, held at the Royal Society last year, and maintained that Prof. W. Ferrel's views on the circulation of the atmosphere, so far as they touch Antarctic winds and pressure, had been misunderstood by Dr. Buchan. Prof. Davis said that it must certainly be clear to every physical meteorologist that the convectional circulation of the atmosphere, as ordinarily stated, was seriously incompetent, for the most striking features in the distribution of atmospheric pressure are not accounted for by it. As long as the effect of the winds in modifying the distribution of pressure is left out of consideration, no broad understanding of atmospheric pressures can be reached.

Statement of the Theoretical Laws of the Polar Motion[*].

I HAVE, during the past seven years, in the 'Astronomical Journal' and elsewhere, stated several theorems relating to the variation of latitude which, when combined, would form the basis of as complete a theory of the subject as at present seems to me possible of construction. I have not, however, been able to put a complete paper on the subject into a satisfactory shape. As the exact relation of what I have said to the observed motion of the pole may not be entirely clear, I shall ask leave to state the conclusions derived from pure theory, without respect to the question of their agreement with observation.

1. Considering the Earth as an elastic solid, covered wholly or partly by an ocean and an atmosphere, in a state of rest, the only motion of the pole would be the so-called Eulerian motion, consisting of a revolution of the axis of rotation around a certain mean pole of figure P_0. This revolution would take place in a circle if the equatorial moments of inertia were equal; in an ellipse if unequal. In the actual Earth there is not likely to be such a difference of these moments of inertia as to give rise to a sensible ellipticity in the motion. The period of the motion cannot be determined from theory, because the mean elasticity of the Earth is unknown. But it cannot be so short as 430 days unless the Earth is more rigid than steel.

2. The Eulerian motion, as thus described, is subject to modifications by movements of matter on the Earth's surface. These changes are of two kinds, statical and dynamical.

3. The statical changes are such as arise from the annual deposit of snow on the continents. These have no direct effect on the Eulerian motion, but only change it by altering the position of the pole P_0, thereby causing the instantaneous motion of the axis R of revolution to change its direction or its velocity. These changes being annual, their result will be represented by an annual

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[*] Reprinted from 'Astronomical Journal,' No. 452.

term in addition to the regular Eulerian motion. Owing to the approximate symmetry of the American and Oceanic continents in the Northern hemisphere, it does not seem likely that the asymmetry of the snow-fall during winter could produce an annual term exceeding three or four hundredths of a second.

4. The dynamical causes are atmospheric and oceanic currents. Were these currents invariable, and were the Earth circular, the pole would, in consequence, revolve steadily and invariably, according to the law stated in A. J. vol. xvi. p. 81. The Earth being a spheroid, the cause in question so acts as to displace the centre of the Eulerian motion from the actual pole P_0 to such a point P′ that, if the pole of revolution R were at P′, the dynamical motion in question would be exactly annulled by the Eulerian motion of R around P_0. The Eulerian motion will then take place around P′ as if this were the pole of figure.

5. Were the currents in question steady throughout all time, the pole P′ would remain invariable. But they are subject to annual variations, the possible amount of which can be only roughly estimated, and the varying momental axis of which must remain absolutely unknown. We must therefore conclude that the virtual pole P′ is subject to an annual change, the *à priori* determination of which is impossible.

6. Were the annual changes in question the same in every year, the actual revolution of the pole would be represented by two periodic terms, the one having the Eulerian period, and the other a period of one year.

7. The annual meteorological changes not being the same from year to year, but yet being invariable in the general average, it follows that the general character and magnitude of the annual term will be permanent, as will also be the period of the Eulerian term. But the effect of accidental differences in the annual meteorological movements from year to year will be to produce changes in the amplitude of the Eulerian motion. Each such change will be permanent in its nature, but constantly subject to increase or diminution by subsequent changes. Hence the amplitude will be subject to slow changes from time to time, which do not admit of prediction, while the apparent period will be subject to small irregular oscillations round a mean value.

Owing to the impossibility of setting exact limits to the possible annual changes in atmospheric currents, it is impossible to say absolutely that any conclusions of this theory are at variance with the deductions from observation by Mr. Chandler, as found in the 'Astronomical Journal.' Whatever motions be observed in the pole, we may assume atmospheric currents which would give rise to them. At the same time, we may set more or less probable limitations to these changes. Considering the probability of the currents in question changing between certain limits, I would lay down the following statements :—

α. It is quite within the limits of ordinary probability that the coefficient of the annual term should amount to $0''\cdot10$. But the probability rapidly diminishes when we suppose it to have a larger value than this.

β. It is extremely improbable, one might say it is incompatible with any probable law of change in the atmospheric movements, that the annual revolution should be in a markedly elliptic path. That is to say, Mr. Chandler's conclusion that the annual term is a very elongated ellipse, is not easily explicable by the probable action of any known cause. The question whether the ellipticity which he finds is real and not due to errors of observation arising from annual and diurnally changing causes, is foreign to the purpose of this paper.

I quite agree with Mr. Chandler as to the eminent desirability of determining the actual polar motion from observations, with entire disregard of any theory. But I think it would be a mistake to conclude that, having laid down these motions, and having approximately represented them by empirical formulas, such formulas are to be taken as expressing the actual law of the motion, irrespective of their agreement with theory. S. Newcomb.

A Determination of Longitude: Leyden—Greenwich.

A section of the volume of the Leyden observations noticed on page 132 of this number is, for several reasons, of so much interest that it seemed scarcely possible to deal with it adequately in the short space to which our reviews are usually limited. It is proposed to give the details here with some comments thereon.

The memoir is a full account of the determination by telegraphic method of the arc of longitude Greenwich—Leyden made in the years 1880–1 by Dr. H. G. van de S. Bakhuyzen and his brother Dr. E. B. van de S. Bakhuyzen. The immediate necessity for the determination was due to the fact that the only value of the longitude of the Leyden Observatory at that time depended mainly on an investigation made in 1837 by Kaiser depending on 10 occultations. A provisional value of this telegraphic determination has been published and referred to several years ago, but we now have before us actual details of the work.

The station at Greenwich was in the south ground of the Royal Observatory, the same that M. von Oppolzer had occupied for Greenwich—Vienna longitude in 1876. This station is 196 feet ($0^s\cdot206$) east of the meridian of Greenwich. The point at Leyden marking the other end of the arc was the centre of the meridian circle of the Observatory, for this was one of the instruments used. The other instrument used for determination of time was a transit-instrument of 2·7 inches aperture and 33·9 inches focal length. For reasons that will be easily understood, the instruments did not exchange stations with the observer, but to obviate any error in the result from this cause the following scheme was carried out. From July 6 to August 12 E. B. observed at Greenwich with the smaller instrument, whilst H. B. worked at Leyden. The observers then exchanged stations, and E. B. observed with the Leyden meridian circle, and H. B. was at Greenwich from August 22 to September 4. Then from December until March following both observers worked at Leyden with the two instruments side by side, each determining the error of the same clock. The two observers interchanged instruments as in the actual longitude, so that by this means any difference of personality due to the instruments was determined and eliminated from the result, which seems to have been the best possible arrangement, as circumstances did not permit of the exchange of instruments.

The Leyden instrument is reversible, but this operation is not effected easily, so that it was reversed only twice during the first period, between the observations on July 30 and August 1, and again between those on August 31 and September 1; each observer thus used the instrument in both positions. The instrument used at Greenwich was provided with a convenient apparatus for reversal, and was reversed sometimes as many as four times during the evening.

This matter of reversal of the instruments and the cognate one of the determination of the collimation is one of the chief causes that has induced us to examine here the details of the work, for there are reasons for thinking that the final word has not yet been said about reversible transit-instruments, and that the subject will bear discussion.

The collimation of the Leyden telescope was determined by means of the level and a mercury-trough, from which it is understood that by means of the mercury-trough the reading of the micrometer was found which indicated a line perpendicular to the surface of mercury; the level gave the inclination to the horizon of a straight line joining the pivots, and by applying the quantity appropriate to this inclination the micrometer-reading was found for a line in the instrument at right angles to the straight line joining the pivots, *when the object-glass was pointing to the nadir.* This micrometer-reading from actual observation was different in the two positions of the instrument. The difference is represented by about $0^s{\cdot}03$ and is quite obvious. We shall refer to this later, and meanwhile we will consider the other instrument.

In this case the collimation was determined by observing a polar star at the same transit in both positions of the instrument, which is equivalent to using a star as a distant mark. It seems that there was no mercury-basin, but that a level only was used. The agreement of the individual determinations of collimation during an evening is surprisingly good, although there is a progressive change during the operations. But adopting the collimation thus found for both positions of the instrument, and deducing clock-error, there is found to be a marked difference in this same clock-error as determined "screw east" and "screw west." The difference varies from night to night, and ranges during the first period from $-0^{s}\cdot054$ to $+0^{s}\cdot140$; and in the mean, clock slow with screw east is greater than with screw west by $0^{s}\cdot068$. On seeing this we referred to the published account of the longitude Paris—Leyden, which was effected a few years later, when Prof. Bakhuyzen used the same instrument; and here we found that the similar difference was $0^{s}\cdot074$ at one station and $0^{s}\cdot033$ at the other, the sign being the same in all cases. Prof. Bakhuyzen adopts always the mean of the determinations as found in the two positions of the instrument as the actual fact, which is probably the most proper course. He attributes the difference to the unequal pressure of the counterpoise springs on the axis; it would be interesting to know whether the transit-stand was in the same position, west and east, in the two longitudes to which we have referred, for this would be an argument for the truth of the hypothesis. Personally we may say that we have some scepticism as to this being the cause, because in certain instruments in use at Greenwich these springs have lately been removed for this reason, but the difference still remains. It will be gathered from the last sentence that the subject of difference of determined clock-error is not new to us; and it is the fact that we have had experience of similar differences, some of much greater magnitude, to which, however, we cannot at this moment more explicitly refer; but it is easy to find similar instances in many accounts of longitude determinations, generally with some attempt at explanation. The results of Dr. Bakhuyzen's carefully made observations give an opportunity, of which we gladly avail ourselves, to make some remarks of a general nature by way of discussion of the subject.

Many years ago Sir George Airy wrote a paper ('Monthly Notices,' vol. xxv. no. 3) on the effect of flexure on a reversible transit-instrument, in which he showed, from dynamical considerations, that if the axis or the tube of a transit were affected by flexure its line of collimation, properly determined, described a great circle, whose plane was not at right angles to the axis of rotation, and if the instrument were reversed that this circle was the same in both positions of the instrument. Assuming the truth of this, the observations with the Leyden circle appear to indicate such a flexure, for the line of collimation determined as above described would be different in the two positions of the instrument, and this actually was found to be the case. Neither line would describe a great circle. The level would be properly determined, and since the azimuth is that of the small circle described by the imperfect line of collimation, there is little error introduced in the final result.

Still adopting the result of Airy's paper, the collimation determined by double observation of a polar star is a correct determination; but if the instrument has a flexure, the level determined by a striding-level alone does not give the proper errors to be applied to the observations. We think that the method of level-determination may be a possible source of weakness in the result of this longitude. It seems advisable always to make observations at the surface of mercury, as well as to use a level.

This level-determination, however, does not affect the difference of clock-error with screw east and screw west—we are omitting all consideration of pivot-error, which was determined and allowed for. The logical conclusion from the difference of clock-error in two positions seems to be that the great circle described by a line of the instrument is different in the two positions, and hence that the line whose azimuth is determined in the two positions is not the same line, so that it is not reasonable to take a mean of determinations in both positions. This is in opposition to general practice, which follows the theory set down above as to the coincidence of the instrumental meridian in both positions, and it may be added that the final result will not be affected whichever procedure is adopted; but it seems logical to treat each position of the telescope as a distinct instrument, and to determine its errors separately and as well as possible. The main difficulty is the determination of collimation, which is best done by a pair of opposing collimators, a method not always practicable. It will be remarked that if the line of collimation thus determined agrees with that determined from a distant mark, this is evidence that the false meridian is the same in both positions. Finally, may we hazard a suggestion for which, however, we have no proof to offer? In a recent publication discussing the R—D correction, Prof. Eastman suggests that it is due to a straining or displacement of the object-glass; the difference East—West has some analogy to the R—D correction; and though we do not suggest that it is due to looseness or shifting of the object-glass, we think it not unlikely that the cause of both idiosyncrasies will be found at that end of the instrument—that there may be optical reasons which displace the line of collimation according to laws which do not permit it to describe a great circle, as is the general assumption. It is possible that there is something yet to learn about the optics of such an apparently simple instrument as the transit.

To return to the work before us. Signals were exchanged twice or three times during each evening. The telegraph-line consisted of a cable and two land-lines, not quite symmetrical. We assume there was a relay at each end of the cable; but this is not stated, and it would have been of interest to know whether the current sent from each end was of the same sign. The differences of clocks, as shown by signals sent from Leyden and from Greenwich, which gives the "time of transmission," show some anomalies. The time of transmission varies from about five-hundredths of a second to thirteen-hundredths. The last-named quantity is somewhat large compared with that found in more recent operations, which show a rate of transmission of about $0^{s}\cdot01$ per hundred miles, and this line cannot be much more than 300 miles; but it is to be remembered that improvements in telegraphy in the way of fast-speed relays have been effected since 1880. However, the mean result of 18 evenings' work in the first part, E. B. being at Greenwich and H. B. at Leyden, gives for the difference of longitude, equal weight being given to each night,

$$17^{m}\ 56^{s}\cdot144.$$

In the second period, when the observers were interchanged, the mean result of 11 nights is

$$17^{m}\ 55^{s}\cdot627.$$

From which it follows that the difference of personal equation H. B.—E. B. = $+0^{s}\cdot258$.

In the after part of the operations, when both observers were at Leyden, E. B. using the meridian circle and H. B. the portable instrument, the mean of 9 nights' observations gave as the difference of the error of the same clock as found by the two instruments (Portable Instrument—Meridian Circle) $+0^{s}\cdot383$; and when the observers interchanged instruments, the mean of 9 nights gave for the same quantity $-0^{s}\cdot203$: so that the determined difference of longitude between the two instruments was $0^{s}\cdot090$, which agrees excellently with the geodetic longitude, as the portable instrument was 29·05 metres ($0^{s}\cdot102$) to the east of the fixed instrument. This fact, we think, supplies admirable proof of the accuracy of the whole method, and gives considerable confidence in the result. It will be seen that from these observations at Leyden the difference of personal equation H. B.—E. B. = $+0^{s}\cdot293$.

These are the facts of the observations; there are many ways of combining them to show a final result. Prof. Bakhuyzen naturally prefers to adopt the personal equation $+0^{s}\cdot293$, as it includes the P. E. due to instruments. He gives the series of observations when H. B. was at Greenwich a greater weight than the earlier series, and so deduces as the difference of longitude between the meridians of the two observatories

$$17^{m}\ 56^{s}\cdot10.$$

We have mentioned above a determination of the longitude Paris—Leyden, made in the year 1884 by Prof. Bakhuyzen and Col. Bassot. The value found for the arc was $8^{m}\ 35^{s}\cdot213$; and combining these to find the third side of the triangle, we have for the difference of longitude between Greenwich and Paris $9^{m}\ 20^{s}\cdot89$.

H. P. Hollis.

Σ 1263.

This pair of stars, which occurs in Struve's catalogue, is interesting for several reasons, and is in danger of being neglected by observers by reason of its increasing separation. According to Struve, the components are 7·6 and 8·2 in magnitude, and it therefore furnishes an example of a large proper motion in a faint star. That the relative motion is due to the proper motion of the principal star there is no doubt.

W. Struve himself remarked its rapid motion and discussed the then existing observations in the 'Positiones Mediæ,' where the current number is 1032.

From his micrometric measures, extending over the period 1828·26 to 1850·62, he deduced the following formulæ for the relative motion of the pair:—

$$\begin{aligned}\Delta\,\text{R.A.} &= 2''{\cdot}579 + 0''{\cdot}2587\,(t-1839),\\ \Delta\,\text{Dec.} &= 11\ {\cdot}806 + 0\ {\cdot}6544\,(t-1839),\end{aligned}$$

and from the available meridian observations he deduced for the principal star a proper motion in

$$\text{R.A.} = -0^{s}{\cdot}0312 \quad\text{and}\quad \text{Dec.} = -0''{\cdot}566,$$

and consequently for the companion

$$\text{R.A.} = -0''{\cdot}087 \quad\text{and}\quad \text{Dec.} = -0''{\cdot}088.$$

Otto Struve combined his own measures with those of W. Struve and thus extended the period of micrometric measures to 1870·63. From this series he obtained

$$\begin{aligned}\Delta\,\text{R.A.} &= 5''{\cdot}503 + 0''{\cdot}2646\,(t-1850),\\ \Delta\,\text{Dec.} &= 19\ {\cdot}054 + 0\ {\cdot}6554\,(t-1850).\end{aligned}$$

Struve observes that these formulæ satisfy the observations well, but thinks that an additional term depending on the square of the time might be an improvement.

A discussion of the measures down to 1895 gives the following formulæ for relative motion:—

$$\begin{aligned}\Delta\,\text{R.A.} &= 5''{\cdot}53 + 0''{\cdot}2696\,(t-1850),\\ \Delta\,\text{Dec.} &= 19\ {\cdot}04 + 0\ {\cdot}6520\,(t-1850),\end{aligned}$$

or $0''{\cdot}700$ on a great circle in position-angle $22^{\circ}{\cdot}5$.

So close an agreement naturally led to a search for proper motion, and the choice had to be made between

$-0^{s}{\cdot}0275$	and	$-0''{\cdot}595$	Argelander, Romberg,
$-0\ {\cdot}0292$	and	$-0\ {\cdot}592$	Stumpe,
$-0\ {\cdot}025$	and	$-0\ {\cdot}63$......	Porter,

a plain order to the enquirer to compute new values. Using a few more observations than Porter had at command, the following agreeable result was obtained:—

$$-0^{s}{\cdot}025 \quad\text{and}\quad -0''{\cdot}611,$$

or $0''{\cdot}671$ on a great circle in position-angle $204^{\circ}{\cdot}5$.

This is, of course, subject to a much larger correction than the resultant relative motion, and it would be quite within the limits to assume the two results, viz.:

$0''\!\cdot 671$ in position $204^{\circ}\!\cdot 5$
$0\ \cdot 700$ „ $22\ \cdot 5$,

as identical.

This star, therefore, might with advantage be observed for parallax, or better still photographed for parallax.

Although now separated some 55″ they were only $2''\!\cdot 0$ apart in 1821. At the time Lalande observed the principal star it was 16″ following the fainter, instead of, as now, 55″ preceding.

The position of the principal star, which is Lalande 17161, for 1900 is:

R.A. ...... $8^{h}\ 38^{m}\ 35^{s}$
Dec. ...... $+42^{\circ}\ 3'\ 6''$

T. Lewis.

The Benjamin Apthorp Gould Fund *.

On November 17, 1897, the sum of twenty thousand dollars was given to the National Academy of Sciences, as Trustee, to establish a fund to be known as the Benjamin Apthorp Gould Fund, in memory of the father of the donor, Miss Alice Bache Gould, the income to be used to assist the prosecution of researches in astronomy, the administration of this income, in accordance with the terms of the Trust and of a letter of instructions from the donor, to be under the direction of the undersigned.

A sufficient available income has now accrued from the Fund to warrant beginning its distribution, and the Directors are prepared to receive and consider applications for appropriations. As a guide in framing such applications it is desirable to present briefly, but in close adherence to the exact terms of the Trust and of the letter of instructions accompanying it, the principal provisions to be regarded in the administration of the Fund.

The objects of the institution are, first, to advance the science of Astronomy; secondly, to honour the memory of Dr. Gould by ensuring that his power to accomplish scientific work shall not end with his death. In recognition of the fact that during Dr. Gould's lifetime his patriotic feeling and ambition to promote the progress of his chosen science were closely associated, it is preferred that the Fund should be used primarily for the benefit of investigators in his own country or of his own nationality. But it is further recognized both by the donor and the Directors that sometimes the best possible service to American science is the maintenance of close communion between the scientific men of

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* From the 'Astronomical Journal,' No. 453.

Europe and of America, and that therefore, even while acting in the spirit of the above restriction, it may occasionally be best to apply the money to the aid of a foreign investigator working abroad.

The wish was also expressed by the donor that in all cases work in the astronomy of precision should be given the preference over any work in astrophysics, both because of Dr. Gould's especial predilection and because of the present existence of generous endowments for astrophysics.

Finally, the Benjamin Apthorp Gould Fund is intended for the advancement and not for the diffusion of scientific knowledge, and is to be used to defray the actual expenses of investigation, rather than for the personal support of the investigator during the time of his researches, without absolutely excluding the latter use under the most exceptional circumstances.

Application for appropriations from the income of this Fund may be made informally by letter to any of the Directors undersigned, stating the amount desired, the nature of the proposed investigation, and the manner in which the appropriation is to be expended. If favourably considered, a blank for formal application will be forwarded for signature, with the rules adopted by the Directors for the administration of the Fund, and to which the applicant will be expected to subscribe.

Lewis Boss.
Seth C. Chandler.
1899, January 1. Asaph Hall.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Boguslawski, Father and Son.

Gentlemen,—

When two members of the same family have written papers or made contributions to the same department of science, it has sometimes occurred that their separate publications have been catalogued together and attributed to the same person. Carefully as has the Royal Society's list of scientific papers been put together, it is not free from error in this respect, as reference to the name mentioned above will show. All the papers in the list are attributed to Georg von Boguslawski, though two of them were written before he was born, and many others when he was very young, being, in fact, the productions of his father, Prof. Palon Heinrich Ludwig von Boguslawski.

A biographical notice of the latter is given in vol. xii. p. 82 of the 'Monthly Notices' and vol. xxi. p. 177 of the 'Memoirs' of the Royal Astronomical Society, of which he had been elected an Associate. He was the discoverer of the first comet of 1835.

The last paper published by him appeared in the 'Astronomische Nachrichten,' vol. xxxi. col. 237, giving an account of some occultations of stars observed at Breslau in the month of September 1850. He died there on the 5th June, 1851, the month before the occurrence of the total solar eclipse of that year, respecting the preparations for which he had written in his serial publication 'Uranus.'

His son, Georg Heinrich von Boguslawski, was born on the 7th of December, 1827, and, after filling various scholastic appointments, became in 1874 editor of the 'Annalen der Hydrographie' and of the 'Nachrichten für Seefahrer' in the hydrographical office of the Imperial Admiralty at Berlin. Of the forty papers attributed to him in the first part of the Royal Society's list, only the last four were really his. They are all accounts of meteoric observations; but he also wrote a separate treatise in 1857, 'Die Cometen und ihre Bedeutung als Weltkörper,' and translated in 1871 Schiaparelli's treatise on an astronomical theory of shooting-stars.

The latter is mentioned as the last of the five papers attributed to him in the second part of the Royal Society's list; in the third part five are also catalogued on subjects in climatic meteorology and physical geography, the last of which appeared in 1881. He died on the 4th of May, 1884.

I should mention that my attention was drawn to the above error in the first part of the Royal Society's list by Mr. Griffith, Assistant General Secretary of the British Association, through Mr. Wesley, Assistant Secretary of the R. A. S. We all have to make such frequent reference to those lists that probably you will think it worth while to enable your readers to make the correction in question.

Yours faithfully,

Blackheath, 1899, Feb. 1. W. T. LYNN.

The South Polar Regions of Jupiter.

GENTLEMEN,—

During the approaching opposition of Jupiter the south pole of the planet will be inclined to the Sun and Earth to nearly its fullest extent, so that an unusually favourable opportunity will be afforded for detecting any spots or markings in high southern latitudes; and I wish to call the attention of observers to the great importance of securing observations of any such spots that may be visible, with the view of extending our knowledge of the rotation-period, or drift, of the surface-material of these southern regions. With this object the part of the disc adjacent to the south pole should be carefully examined on every fine night, and in the event of any definite spot being observed, every effort should be made to note the times when it is in mid-transit across the disc as exactly as possible. If practicable, micrometer-measures of its distance from the N. and S. limbs would also be very desirable, in order that its latitude may be exactly ascertained. Observations made systematically on every fine night would pretty certainly reveal the presence of some definite spot or other marking, for such objects are not very rare.

The published determinations of the rotation-period of markings in the higher southern latitudes are not very numerous at present, as will be seen from the following list:—

	Latitude.	Rot. period. h m s	
1886......	−50°	9 55 11·1	C. A. Young.
1888......	−37° to −55°	0·9 (2 spots)	A. S. Williams.
1890......	−36 to −45	6·7	„
1892......	+32 to −39	8·4	„
1898......	−40° ±	6·3 * (2 spots)	T. E. R. Phillips.

The extreme southern limit is −55°, but, if circumstances only prove favourable, there should be no difficulty in extending this to −75° at least. It is true that the inclination of the planet's axis will be but small, not much over 3°; but, nevertheless, even this small inclination will be a material help in observing any markings near the south pole. In the more central parts of the disc its effect at such times is to cause the belts to appear distinctly curved, though very few observers appear to have noticed this fact or to represent it in their drawings. The curvature at such times is nevertheless very striking if a micrometer-wire be placed parallel to the belts, whilst even without such aid it is obvious enough. It is rather remarkable that some observers, who have failed altogether to notice or describe this evident curvature of the *very conspicuous* belts of Jupiter, have been the loudest in declaiming against the reality of the canals of Mars, because these *immensely* more delicate streaks have sometimes been drawn straight when they must actually have appeared curved! The Lick photographs of Jupiter, taken at the time when the N. pole of the planet was inclined towards the Earth, show the curvature of the belt in a most striking manner, so as to make one wonder that so obvious a feature could be overlooked by anyone.

Yours faithfully,

Hove, 1899, Feb. 15. A. STANLEY WILLIAMS.

Young's 'General Astronomy.'

GENTLEMEN,—

On the occasion of my visit, a few days since, to the rooms of the Royal Astronomical Society, I saw the new and revised edition of Prof. Young's classical work on 'General Astronomy,' and on examination of it found that, in addition to certain numerical corrections of constants, &c., it contained a short, but none the less valuable, Appendix. Now why should not the publishers issue these additions and corrections in the form of a pamphlet for binding up with the original edition? Of course, from a bookseller's point of view, it might (though I gravely doubt if it would) pay better to attempt to compel possessors of the first edition to buy the volume over again; but it is not a very cheap book, and many who, like myself, will be content to consult the new edition at rare intervals in the R. A. S. Library, would cheerfully pay a shilling or two for the additional matter in a separate form. *Verbum sat sapienti.*

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* Mr. W. F. Denning, from his own observations alone of the same two spots, makes the rot. per. $9^{h}\ 55^{m}\ 8^{s}{\cdot}6$. Mr. Phillips's result is based on all the available observations.

Forest Lodge, Maresfield, Uckfield.
1899, Feb. 16.

Yours faithfully,
William Noble.

Dr. Cobham Brewer and the Galactic Circle.

Gentlemen,—

One frequently meets with amusing instances of misconception by writers when treating of subjects out of their own line. But I have seldom come across one more so than that of the late Dr. Cobham Brewer in the last edition (1895) of his handy 'Dictionary of Prose and Fable.' Under "Galactic Circle" he says: "The Galaxy being the sidereal equator, the Galactic Circle is inclined to it at an angle of 63°." This arises from a misunderstanding of Sir John Herschel, who was the first, I believe, to use the expression "Galactic Circle," meaning by it the great circle to which the Milky Way in its course most nearly conforms, and which, as it happens, makes an angle of about 63° with the equinoctial. The Galactic Circle is, in fact, the Galaxy at its central line, so far as it has one; Herschel remarks that "it is to sidereal what the invariable ecliptic is to planetary astronomy —a plane of ultimate reference, the ground-plane of the sidereal system." This Dr. Cobham Brewer interpreted into meaning that the galaxy and the galactic circle were related to each other similarly to the celestial equator and equinoctial, though even then it does not seem to have occurred to him that these are inclined at an angle, not of 63°, but of 23½°, which is nearly its complement.

Yours faithfully,
Blackheath, 1899, Jan. 2.
W. T. Lynn.

P.S.—Perhaps I may here give an instance of how mistakes are sometimes made through a desire to be accurate, but not seeking to obtain this in a judicious way. In the bibliography at the end of the above work, the author gives, under Airy, *November* 27 as the date of his birth. I wrote to ask Dr. Brewer why he had given a date which was unquestionably erroneous. His reply was that, having seen June 27 mentioned in some places and July 27 in others, he referred to his publisher, who told him that he believed the correct date was November 27. Now had he given either of the others, he might have been right, Airy having been at one time disposed to accept June instead of July 27 from a parochial register. But (as I heard him say himself) his last conclusion was (the register being badly written) that it was preferable to fall back upon his mother's record of the day, which was July 27.—W. T. L.

OBSERVATORIES.

HARVARD.—Prof. E. C. Pickering's Report for the twelve months ending 1898 September 30 opens with a short reference to the lamented death of Prof. W. A. Rogers, for nearly 20 years an officer of the Observatory, and, though occupying at the time of of his death another post, still furnished valuable help to the institution.

The Observatory has been enriched by two benefactions, amounting in all to 45,000 dollars, under the wills of Miss C. M. Haven and Miss E. A. Haven, the income of which may be used for direct purposes connected with astronomical science at Harvard, but there are no further restrictions.

The following paragraphs, which are copied *verbatim* from the Report, may be found interesting:—

> As the organization of the Harvard College Observatory is unlike that of many similar institutions, it may be worth while to describe it. In large observatories it is not unusual to establish a number of departments, each under the entire charge of an astronomer who is often unaided by assistants. The institution thus becomes a series of small observatories under one roof, but in other respects quite independent. The advantages of this plan are, first, the education of a group of astronomers of the highest grade, each of whom is enabled to devote his entire energy to his work, and the illness or failure of one in no way diminishes the efficiency of the others. On the other hand, there is often a lack of co-operation—it is a ship in which all the sailors are captains. It is not clear that better results are thus obtained with a given expenditure of money than if assistance was given to amateurs who had displayed especial skill in their work, either by furnishing them with suitable instruments, or means for publication. The power of the whole is not greater than that of as many detached small observatories.
>
> The Director of the Harvard College Observatory takes immediate charge of the various departments, in many cases making a daily inspection and planning the work in detail. Many of the assistants are skilful only in their particular work, but are nevertheless capable of doing as much and as good routine work as astronomers who would receive much larger salaries. Three or four times as many assistants can thus be employed, and the work done correspondingly increased for a given expenditure. This method does not offer the same opportunity for the advancement of individuals, and too much depends upon a single person—the Director. The advantages for co-operation, or for undertaking large pieces of work, are very great, and the latter is especially marked since this Observatory maintains stations in the southern as well as in the northern hemisphere. The same plan of work can thus be carried out for all stars from the north to the south pole, and the experience gained at one station greatly aids the work at the others. Moreover, uniformity of plan of publication is secured, and on special occasions, as during eclipses, meteoric showers, &c., large numbers of skilled observers are available on short notice. Each

method has its advantages, and it seems advisable that this method should continue to be followed in one large observatory.

The division into departments referred to will be understood from the following abstract of the Report of work done in each of them :—

East Equatorial, 15-*inch*.—Mr. O. C. Wendell is in charge of this, and has two assistants. This telescope has been used for photometric work, and is furnished with a polarizing photometer with achromatic prisms, which is used for comparison of the light of variable stars of long period, and also with a second photometer adapted to the comparison of stars too close together to be measured with the first-mentioned instrument. With this instrument also photometric observations of the eclipses of Jupiter's satellites have been made, in continuation of a long series hitherto made. The total record for the year is about 25,000 comparisons.

West Equatorial, 6-*inch*. Mr. Reed, assisted by Miss Cannon and Mr. Waite.—This instrument has also been used for photometric observations of variable stars, and with it about 1700 estimates have been made.

Meridian Photometer. — This is still used exclusively by the Director, who, on 152 nights, has made 73,684 photometric settings. The work on the northern stars planned for this instrument is now completed. This includes the re-observation on three nights of all the stars in the Harvard Photometry. In connection with photometric work it is mentioned that a series of observations with the 12-inch telescope mounted horizontally has been begun by the Director to determine the magnitude of the fainter stars. The star whose light is to be measured is reflected into the field of the telescope, and then compared with an artificial star made by a Welsbach burner shining through a small hole, the light of which is varied by means of a wedge of shaded glass until it is equal to the real star.

The Henry Draper Memorial.—The work of this department, which, as well as other Astrographic work, is under the charge of Mrs. Fleming, assisted by twelve ladies (the photographs are taken by Mr. E. S. King and two Assistants), has been pursued with accustomed vigour. Some results of this have been already noticed in Harvard College Circulars. We note in the Report that a photograph of α Canis Maj. was obtained in the middle of the day under conditions which render it probable that bright stars could be usefully photographed in the daytime in a transit-instrument or while undergoing occultation.

The telescopes at Arequipa, which are in the charge of Prof. Bailey, have been devoted, as in former years, to photographs of clusters and other astrographic work. The telegraphic longitude of this station has been determined.

It will be gathered from the above that the work at Harvard is chiefly photometric and photographic. The meridian work, under the direction of Prof. Arthur Searle, with five assistants, four of whom are ladies, has consisted of the completion of the reduction of observations of fundamental stars begun by Prof. Rogers, and the completion of the observations for and formation of the zone-catalogue of the Astronomische Gesellschaft −10° to −14°. These observations are completed, and the catalogue is proceeding satisfactorily.

Arrangements are already being made for observation of the Leonids next November from various stations.

PUBLICATIONS.

THE WASHINGTON STAR CATALOGUE*.—Professor Eastman's catalogue contains 5151 stars based on 72,914 observations, extending over the twenty-five years from 1866 to 1891. A preface by Professor Harkness, the astronomical director, makes the surprising statement that the catalogue "has absorbed the labours of about two-thirds of the Observatory staff for more than thirty years." This would seem to imply that the available force is weaker than is commonly supposed. At the end of the catalogue is given a table showing the length of service of each observer, and the number of observations made by him and used in the catalogue. The ratio of the number of observations to the time has a very wide range in the cases of different observers.

Of the 3222 Bradley stars, we infer, from a count here and there, that some 1350, or about four-tenths, are to be found in the catalogue. The remaining 3700 range widely in magnitude and position.

The stars of the catalogue are sharply divided into two classes, treated in wholly different ways—fundamental or ephemeris stars, and miscellaneous stars. The latter are roughly about 5000 in number, and their positions depend on 29,556 observations, so that the average number for each star is about six.

The first section of the catalogue proper gives the separate annual results for the Right Ascensions and North Polar distances of the fundamental stars. In Section 2, the mean result for the position of each ephemeris star is separately derived from direct and reflected observations and from transits above and below the pole. Next follows a section giving the separate annual results for each of the miscellaneous stars. The last section continues the catalogue proper, giving the concluded position of each star of both classes, with the usual data of reduction.

The work is so conducted that in the reduction of the ephemeris stars, from the dates of observation to 1875, proper motion is included, while it is omitted in the case of the miscellaneous stars.

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* 'The Second Washington Catalogue of Stars, together with the Annual Results upon which it is based; the whole derived from Observations made at the United States Naval Observatory with the 8-inch Transit-Circle during the Years 1866-91, and Reduced to the Epoch 1875.' Prepared under the direction of John R. Eastman, Professor of Mathematics, U.S.N. Washington, 1898.

The methods of observation and reduction are fully set forth in a very copious introduction of sixty-two pages. We infer from Professor Eastman's statements that he has found it necessary to reconstruct or revise nearly all the results found in the annual volumes.

No Right Ascensions of fundamental stars were determined during the years subsequent to 1875. We find some difficulty in reconciling this omission with the reasons for it found on pp. xvi and xvii, which are, in substance, that *before* 1875 the conditions were in many ways unfavourable for such determinations. It would seem that determinations were made when the conditions were unfavourable, and omitted when it is implied that the conditions and methods were improved.

The most striking feature of the work is the large probable error of the annual results in declination, arising from the instability of the instrument.

The discordances between the results from direct and reflection observations are singularly large, frequently reaching 2″ in the mean for an entire year. The observations are reduced on the supposition that R—D has one constant value north of the zenith, and another south of the zenith, there being a discontinuity in passing the zenith. But the amount of the correction, as well as of the discontinuity, varies very widely from year to year. In 1880 the discordance is 5″ for stars north of the zenith, and 2″·6 for stars south of it, so that the discontinuity amounts to 2″·4. This, however, is an extreme case. The algebraic signs of the discordances generally alternate from year to year, owing, we suppose, to an annual reversal of the instrument.

Professor Eastman (p. xxx) attributes this fault to an instability in the mounting of the objective in its cell, resulting in its tilting as the instrument is moved in different positions. He conceives that this tilting was greatest when the instrument was pointed downwards to take reflected observations, a conclusion which seems justified by the greater discordance of the latter. At the same time the weakness might equally affect the direct observations, and the discordances among the annual results in Section 1 lead us to suppose that such was the case. The polar distances of the fundamental stars, even those most frequently observed, nearly always range over more than 1″. Thus, in the very favourable case of Polaris, out of 22 annual results from observations above the pole, each resting generally on 40 observations or more, we find that

4 results are 26″·4 or greater,
4 „ 25·33 or less.

It ought to be added that the results *sub polo* agree better, the range being only $1''{\cdot}0$. In case of other fundamental stars the range is nearly always $1''$ or more.

Apart from this unavoidable source of weakness, the material appears to be very carefully and skilfully worked up. The instability of the instrument does not seem to affect the Right Ascensions, and the Declinations are impaired only by the large mean error of individual observations.

Several of the observers determined their personal equation for faint stars by the use of the wire screen. The process by which the result is reached is so combined with that of determining the difference between dark and bright wires as to be, in some points, not quite clear to us. Most remarkable is the value of the correction found by Professor Eastman for himself, which amounts to $0^s{\cdot}21$ for a star of the eleventh magnitude, and to half a second for one of the twelfth.

The correction for equinox is derived by the rather laborious method of computing the Sun's absolute Right Ascension from each observed declination. The only objection to this method, apart from its prolixity, is that observations can be used only near the equinoxes, the average annual number included being 31. The table of separate results and their combination on p. xlvi is quite consistent with other recent determinations of the equinox; but we cannot exactly reproduce Professor Eastman's numbers from the data he gives in Table 17.

As a whole, notwithstanding the drawbacks we have mentioned, the catalogue is a monument of Professor Eastman's ability and untiring industry, on which he may look with a satisfaction that can be qualified only by circumstances over which he had no control. It is interesting to note that the catalogue was completed on the eve of his compulsory retirement from active service. This and the defects of the instrument may account for the absence of special discussions and comparisons of results, such as those pertaining to refraction and latitude. There is, however, a comparison of declinations with those of standard catalogues showing a good general agreement.

LEYDEN ANNALS, VOL. VII.—Dr. Bakhuyzen has been kind enough to send us this recently-published volume, which has supplied us with some excellent reading. It consists of four researches in different subjects, each of equal value. The first is a re-determination of the rotation-period of Mars. Drawings of ancient and modern observers were compared and discussed, from Hooke, 1666, to Schiaparelli, 1879, the complete list of authorities used as data being, in order of time, Hooke, Huyghens, Herschel, Schroeter, Mädler, Rosse, Lockyer, Kaiser, Mesten, Dreyer, Green, Lohse, Schiaparelli. The rotation-period resulting from the discussion is $24^h\ 37^m\ 22^s{\cdot}66$, which compares with the value $24^h\ 37^m\ 22^s{\cdot}71$ found by Proctor, and $24^h\ 37^m\ 22^s{\cdot}62$ by Marth. The author points out changes that have evidently taken place since Schroeter's drawings were made.

The next section is a description and account of observations made with the Personal Equation Machine at Leyden, which resembles that in use at Greenwich, where an artificial star is made to travel at different speeds across the field of the transit-circle, its times of passage over the wires being recorded automatically and by the observer.

There then follows a determination, by Prof. Kapteyn, of the parallax of 15 stars from meridian observations. The concluding section of the volume we have dealt with at some length on another page.

NOTES.

Comet Notes.—The following elements of Chase's Comet (1898 VIII.) are by Mr. Coddington from his observations of Nov. 23, Dec. 7, Dec. 16. They satisfy the observations up to Jan. 19 with no error exceeding 1^s or thereabouts (Ast. Nach. 3544):—

T	1898 Sept. 20·1534 G.M.T.	
ω	4° 38′ 0″	1899·0
☊	95 51 36	
i	22 30 20	
log *q*	0·35889	

Ephemeris for Greenwich midnight.

	R.A.	N. Dec.		R.A.	N. Dec.
	h m s	° ′		h m s	° ′
Mar. 2....	10 55 35	38 4	Mar. 22 ..	10 46 42	38 17
6....	10 53 29	38 16	26 ..	10 45 36	38 7
10....	10 51 30	38 23	30 ..	10 44 47	37 53
14....	10 49 40	38 25	Apr. 3 ..	10 44 17	37 37
18....	10 48 4	38 23	7 ..	10 44 8	37 18

The brightness is steadily declining, being half that at discovery on March 30.

The following continuation of the ephemeris of Wolf's Comet is by Thraen (Ast. Nach. 3544) for Berlin midnight:—

	R.A.	S. Dec.		R.A.	S. Dec.
	h m s	° ′		h m s	° ′
Feb. 28....	6 17 28	8 29	Mar. 12....	6 24 11	6 28
Mar. 4....	6 19 24	7 47	16....	6 27 0	5 50
8....	6 21 39	7 7	20....	6 30 4	5 15

Mr. Perrine describes (Ast. Nach. 3546) an unsuccessful search for Tempel's First Periodic Comet (1867 II.), made at the Lick Observatory in June and July last. The entire region was swept over twice with the 36-inch; many new nebulæ were found, but not the comet.

Mr. Merfield gives the following hyperbolic elements of Comet 1898 VII. (Coddington-Pauly) in Ast. Nach. 3546. They are based on observations extending from June to October 1898:—

T	1898 Sept. 14·05216 G.M.T.	
ω	233° 15′ 36″	1898·0.
☊	73 59 21	
i	69 55 57	
log q	0·230828	
e	1·000753	

A. C. D. C.

MINOR PLANET NOTES.—Two new planets, EE, EF, of the eleventh magnitude, were discovered by Wolf and Schwassmann at Heidelberg on Feb. 15, 17 respectively. Several more images of (433) Eros have been found on the Harvard plates of 1893–94, so that material exists for a very precise determination of the orbit.

A. C. D. C.

THE CONSTANT OF ABERRATION.—Messrs. Rees, Jacoby, and Davis have been continuing their observations and researches on the variation of latitude at Columbia University. Results are published in Ast. Journal, No. 451, which are difficult to summarize briefly; but it may be mentioned that the value of the constant of aberration, which is always a bye-product of such researches, comes out as 20″·461 ±0″·006, which is about a mean value of previous determinations.

Prof. Jacoby has another research on hand to determine this constant, the method of which he described at the recent Astrophysical Conference at Harvard. The method is photographic. If a series of short exposures be given on the region of the pole of the heavens, the result will be a series of dots, lying in circular curves, one for each star. The centre of all the circles is the pole of the heavens. Therefore plates taken at different times will show the position of the pole with respect to the stars, and it is hoped that from analysis of this position there may be deduced the variable motion of the pole and the constant of aberration. This is, of course, merely the application of photography to an old principle, and it seems easy in theory. We hope it may be equally easy to effect in practice.

While on this subject we will call attention to a paper by Monsieur Bigourdan in the 'Comptes Rendus' for November 28 last. He there proposed to use an instrument for the determination of the aberration constant, which does not differ essentially from the reflex-zenith tube at Greenwich. Monsieur Bigourdan is no doubt familiar with the results obtained with this instrument; the observations have, however, not been discussed with a variable latitude as a factor. He, however, proposes to observe many stars, instead of one only as at Greenwich, and by this means it is theoretically possible to determine the variation of latitude and the aberration.

THE VELOCITY OF η PEGASI.—It has already been announced (December 1898) that Prof. Campbell found from his observations that the velocity of the star η Pegasi in the line of sight is variable. This has been corroborated by Herr Belopolsky, who, in Ast. Nach. 3536, gives, as the result of his measures at Poulkova, the following velocities of the star relative to the Sun in geographical miles per second:—

1897	Aug. 27....	−0·66	1898	Aug. 25....	+2·29
	Sept. 8....	−0·64		Sept. 17....	+2·26

Reducing these to kilometres to compare with Prof. Campbell's results, they become, in 1897, −4·8 km., in 1898, +16·9 km. Prof. Campbell's similar results for about the same date were −2·2 and +16·5.

Other stars recently found by Prof. Campbell to have a variable velocity in the line of sight are *o* Leonis, whose velocity varies from +66·7 km. per second in 1898 March to −23 km. in November, and χ Draconis, the velocity of which, deduced from measures of spectrum plates taken between July and December of last year, seems to indicate a period of 5 or 6 months, with an amplitude ranging from about +46 km. to about +11 km. per second.

THE CROSSLEY REFLECTOR AT LICK.—It will be remembered that the 3-foot reflector, made by Dr. Common and presented to the Lick Observatory by Mr. Crossley, proved to be the cause of some dissension between the late Director and his Assistants, who all thought the instrument too antiquated to use. Prof. Keeler, the new Director, has restored to this historic instrument its good name, for with it he has taken photographs, with which he is perfectly satisfied, of Comet I. 1898. He says that the reflector only needs a sufficiently powerful guiding telescope to become an efficient instrument for the photography of comets, and this will be supplied as soon as possible.

VARIATIONS IN THE SPECTRUM OF THE ORION NEBULA.—Prof. Keeler, in Ast. Nach. 3541, writes:—"Some doubt having been expressed as to the reality of variations which have been observed in the spectrum of the Orion Nebula, I made some visual observations with reference to this question on the night of December 12, with a spectroscope attached to the 36-inch refractor.

"The slit was first placed on the nebulosity surrounding the star Bond 734. The night being hazy, only a single line was visible. It was occulted by the coarse micrometer wire, and, on throwing in the light from a hydrogen tube, was identified as Hβ.

"The slit was then placed on the Huyghenian region, near the trepezium. The usual spectrum was well seen. Hβ and the second nebular line ($\lambda = 4959$) were about equally bright; the chief line ($\lambda = 5007$) was several times brighter than either.

"The intensity of the spectrum was then diminished by contracting the vertical aperture of the spectroscope, the resolving power remaining therefore unchanged. When the brightness was sufficiently reduced, Hβ and the second line disappeared, the chief nebular line being then alone visible.

"In other words, with a sufficiently feeble spectrum the Hβ line was alone visible in one part of the nebula, and the chief line was alone visible in another part. This result is inexplicable on physiological grounds. It can only be due to real differences in the spectrum of the nebula."

A NEW BIBLIOGRAPHY.—We are glad to find space for the following announcement:—

"I intend to publish an 'Astronomische Jahresbericht, mit Unterstützung der Astronomischen Gesellschaft' ('Astronomical Yearly Report, aided by the Astronomische Gesellschaft'). It will give short reports of all the works on astronomy, astrophysics, and geodesy, both practical and theoretical, which have appeared during the year. The first volume will appear in 1900, and will contain reports of all the publications of 1899. Not wishing to overlook anything, I should be much obliged if all authors of such works which appear as single books, or articles in journals which are not usually considered to be astronomical publications, would kindly communicate them to me.—Prof. W. F. WISLICENUS, Ph.D., Strasburg University."

A NEW VARIABLE.—Mr. T. D. Anderson, of Edinburgh, announces (Ast. Nach. 3533) the discovery of a new variable star. The place for epoch 1855·0 is R.A. $23^h\ 27^m{\cdot}5$, Dec. $+55^\circ\ 45'$. The magnitude at the beginning of 1897 appears to have been about 9·2; in 1897 September it was not visible when a neighbouring star, 10·3 mag., was. In 1898 December its brightness appeared about 9·6.

ERROR OF TRANSITS DUE TO MAGNITUDE.—In a paper by Mr. R. H. Tucker, of the Lick Observatory, in Ast. Nach. 3539, there is an interesting fact worth extracting. For the correction to transit depending upon magnitude a series of screen-determinations were made. A first series of 150 transits gave a correction to transit $-0^s{\cdot}009$ per magnitude; a second series of 120 transits $-0^s{\cdot}006$ per magnitude. The sign shows, as has been found in similar investigations, that faint stars are observed relatively later than bright ones. A more remarkable instance of this error will be found in Prof. Eastman's Catalogue, mentioned on page 132 of this number.

The Central Condensation of the Andromeda Nebula.—The following evidence for the non-existence of a stellar appearance in the centre of the Andromeda nebula which has lately been suggested is published by Prof. W. J. Hussey, of the Lick Observatory, in No. 451 of the 'Astronomical Journal':—"On the evening of 1898 September 20 Prof. Campbell and I carefully examined the central part of the Andromeda nebula with the 36-inch refractor, using powers of 270, 520, and 1000. We found the central condensation comparable in brightness to a star of the 11th to 12th magnitude. The most condensed part of the nebula was some 2″ in diameter, with no definite limits, but fading by imperceptible degrees to the general intensity of the surrounding nebula. It was not stellar; its appearance was quite unlike that of a neighbouring star of about the same brightness which appeared in the same low-power field. So far as I can judge, its appearance was the same as on previous occasions when I have examined it with the same telescope."

Prof. Campbell examined the central condensation with a direct-vision spectroscope, and found the spectrum continuous, with no evidence of bright lines, and in all probability the same as that of the rest of the nebula.

In our notices of the annual Report of work done at Continental Observatories published last November and December, which are necessarily brief, we unwittingly did a little injustice to the Turin Observatory. Dr. Porro, the Director, points out that the expression "mostly meteorological" scarcely applies to an institution which is engaged in the work of revising Piazzi's Catalogue. We willingly accept Dr. Porro's correction, merely pointing out that we understood the work had not been begun at the date to which the Report referred. We have no doubt but that, under Dr. Porro's energetic management, this great work is actively progressing.

As usually happens at this time of the year, we have received a copy of a new edition of Mr. Lynn's handy little book, 'Remarkable Comets.' This is the seventh edition. We need scarcely repeat our previous expression as to the usefulness and cheapness of this little work, but will merely add as evidence of the first of these qualities that the book contains a list of the periodical comets which may be expected to return this year: the 1866 comet, connected with the November meteors, Tuttle's comet, Temple's second, Holmes's comet, and Finlay's.

THE Egyptian Government has decided to establish an observatory on the Mokattam Hills, on the site of the station occupied by Capt. Orde Brown's party, for observation of the Transit of Venus, in 1874. It will be remembered that this is one of the stations of a longitude chain joining Greenwich to Madras.

SIR J. N. LOCKYER, F.R.S., has been appointed by the President of the Royal Society to be a member of the Board of Visitors of the Royal Observatory, Greenwich, in the place of the late Rev. Bartholomew Price.

A MONUMENT to the memory of M. Tisserand, late Director of the Paris Observatory, is to be erected in his native town, Nuits Saint-Georges.

FROM AN OXFORD NOTE-BOOK.

THE appearance of a new book * on Earthquakes by Prof. Milne, and an interesting review † of it by Prof. Knott, which has been circulated in pamphlet form, remind us of the growing interest in seismology. The reviewer compares the new seismology with the old: the old earthquakes were so destructive as to overwhelm cities, annihilate islands, and lay waste broad stretches of country; "but within this generation a new seismology has arisen," the study of the gentler earthquakes that spare the work of man, and are "almost welcomed by the enthusiasts who make seismology their special study." Prof. Knott aptly quotes a remark made by a well-known seismologist when he had got a new and improved instrument into working order. "Now," he said, "if a good earthquake would only come along!" There are still many of us, brought up in the old fear of all earthquakes, who cannot hear the remark without regarding it as bordering on impiety. It will, perhaps, take another generation or two to get rid of this ingrained mistrust.

THE best way to eradicate our mistrust of earthquakes and snakes and similar terrors of childhood is, no doubt, to keep them as pets. There seems no reason why the horizontal pendulum for studying earthquakes should not become nearly as popular as the barometer and thermometer for studying the weather, except that in England we get more weather than earthquakes. The late Mr. Latimer Clark wrote his little book on the transit-instrument "for country gentlemen," in the hope that the instrument might become as popular with them as "the stereoscope or the camera." He was, in this matter, a little too sanguine, but no doubt there

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* 'Seismology,' by Professor J. Milne, F.R.S. (Internat. Sci. Series, vol. lxxxv. Kegan Paul.)

† 'Scottish Geographical Magazine' for 1899 January.

were some who profited by his advice. And there must be other country gentlemen who would find it fascinating to read in their own homes the echoes of upheavals in distant Japan. There need not be any calculations as in the case of the transit-instrument. The earthquake-instrument wants winding up regularly, and that is all. Days may pass without any record of interest, but "Milne in his own seismological observatory at Shide, Isle of Wight, obtains records of distant earthquakes at the average rate of about six per month"; so that there is really quite enough excitement for leisure moments. If anyone is considering the desirability of setting up a horizontal pendulum, I may mention that the complete outfit costs something like £30, besides the working expenses of the photographic records.

Dr. G. W. Hill has estimated the mass of Mercury after a novel fashion *. There seemed to him no satisfactory way of choosing between the discrepant values given by its action on Venus and on Encke's Comet, and so he has had recourse to analogy with the Earth and Moon, Mars and Venus. "If there is any truth in the nebular hypothesis the materials forming the masses of the four planets Mercury, Venus, the Earth, Mars, and the Earth's Moon ought to have approximately the same chemical constitution." Assuming Legendre's law of density,

$$\rho = k \sin mr/r,$$

and that m is the same for all bodies, while k is different, Dr. Hill finds the mass of Mercury from analogy with each of the other four bodies, and deduces for the reciprocal of the mass

From Venus	13101500
From Earth	12543000
From Moon	9685750
From Mars	11832400

fairly consistent values, of which he takes the mean. But what a lot of figures! He deduces 11634200 for the mass-reciprocal: six significant figures, the second of which is probably wrong; for it is open to anyone to point to the progressive character of the results from the planets in order of distance from the Sun, and to say that the reciprocal should be between 13×10^6 and 14×10^6 (or even outside the latter), instead of 12×10^3. It is much to be regretted that such high authorities as Dr. G. W. Hill persist in loading astronomical calculations with useless figures.

A correspondent kindly calls my attention to the following

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* Astron. Journal, No. 452.

descriptive note in a story in 'Temple Bar' for December 1898:—

> there suddenly blazed before us low down on the horizon a group of stars I had never seen before, but it was unmistakable.
>
> "The Southern Cross!" I cried in great delight.
>
> "That old kite the Southern Cross! You were never more mistaken in your life!" said my companion, to whom the glories of this sky were equally unfamiliar.
>
> The irreverent comparison was not altogether indefensible; there was a strong resemblance between the child's toy and the most celebrated constellation of the South, with perhaps a truth-compelling leaning toward the kite because of its position, for as the kite is canted from the perpendicular by the force of the wind so do the forces of magnetism draw the cross, and at midnight only does it stand erect.

Airy used to say that very few people know that the stars rise and set; and if the casual person is cross-examined on the point it will indeed often be found that he has neither observed any change of position in the constellations during the night nor thought of the probabilities. But the above instance is curious in that the writer is observant enough to note some of the facts, but yet entirely overlooks the simple and real explanation of them, and has to invent a complex and false one.

The following poem is a genuine Oxford production. It is taken from an undergraduates' periodical called the 'J. C. R.,' which being interpreted means Junior Common Room. A poetical imagination must be a great gift, and it is well for us who are accustomed to observe Sirius at a considerable zenith-distance without smoked glass to look at it for a few moments with a poet's eye and see it "on the zenith, scorching the Earth in tyrannical sway":—

THE DOG STAR.

Like jewels of light in a magic pavilion,
  Glow magnificent stars in the indigo sky,
But in saffron and silver, in gold and vermilion,
  The Dog Star is changing and gleaming on high.

Ardent and dominant, beautiful, brilliant,
  He scorches the Earth, in tyrannical sway,
Till only the billows are cool and resilient,
  Crested and plumed with their silvery spray.

The world is half mad with the heat and delirious,
  For no longer the sibilant breezes come,
Under the luminous circle of Sirius
  Everything living is sullen and dumb.

On the zenith he glows ever glitt'ring and weaving
  His colours in tissues and prisms of light,
A marvellous island of radiance, relieving
  The infinite hollow of cavernous night.

I can hear the long drawn breath of the ocean
  Coming irregular, murmurous, deep,
Weary and tired with monotonous motion,
  Soothing all else but itself into sleep.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1899 March 10.

Prof. G. H. DARWIN, LL.D., F.R.S., *President*, in the Chair.

Secretary: H. F. NEWALL, M.A.

THE Minutes of the last ordinary Meeting were read and confirmed.

Mr. Newall. Since the last Meeting, 120 presents have been received, of which I would call attention to the following:—'A Short History of Astronomy,' by Mr. Arthur Berry; from the Rev. Father de Campigneulles, 'Observations taken at Dumraon in India during the Eclipse of 1898, January 22'; 'A Treatise on Photographic Optics,' by R. S. Cole; from Dr. Fison a copy of his book 'Recent Advances in Astronomy'; from the Columbia University Observatory, New York, a volume of various memoirs connected with the Rutherford photographic plates; from Mr. Salmon, photographs of artificial Lunar forms. Dr. Spitta has kindly sent a copy of his book on Photo-micrography, in hopes, no doubt, that the matter dealt with will be of use to those who are engaged in astronomical photography. Prof. Max Wolf sends photographs of fields of stars showing trails of minor planets, &c.; these are copies on glass. There has also been received a photographic print of the Corona from Mr. Burckhalter, taken with his special shutter, and we hope that Mr. Wesley will give us a short account of this photograph. I ought to have prefaced my remarks by saying that Mr. Dyson, owing to indisposition, is unable to be present at the Meeting this evening.

Mr. Wesley. I will read a passage from Mr. Burckhalter's letter, in which he explains the photographs:—"I send you to-day two pictures of the eclipse of January 1898. The print shows details but poorly, and the extension also fares badly. The total exposure was 4 seconds, while at the Moon's limb the exposure was 0·08 sec. The total should have been 8 to 10 secs., and the extreme inner corona should have received $\frac{1}{300}$ or $\frac{1}{400}$. The zone around the corona was my fault. I hope Fellows will be charitable with regard to a new invention." I think you all know that Mr. Burckhalter worked with a new instrument designed by himself, which consists of a revolving shutter with the aperture so shaped that the inner part of the corona should have a short exposure, and that the exposure should gradually increase towards the outer part. The idea was to obtain a picture of the corona giving its faint extensions without over-exposing the limb. Unfortunately, the print only arrived yesterday, and I have not had time to make any slides of it, but I have made a hasty sketch on the blackboard, to give some idea of the amount of extension of the corona, which is very remarkable, especially when we consider the large size of his image. He had a focal length of 15 ft. The plates, I believe, are contact prints, and the image of the Moon on the pictures is nearly 1¾ inch. The great extensions reach, one to one and a half, and another to two diameters. It appears from the pictures that he did not get the Moon strictly centred. These things must be allowed in the first trial of a new instrument. A very little examination shows that for such a purpose the method is exceedingly promising. I do not think I ever saw so great an extension with so large an image, and at the same time so much detail in the lower parts of the corona. Close to the three prominences on the N.W. quadrant the corona shows much detail. One fine ray appears to spring from the Sun and bend completely over towards the limb. With regard to any theory of the corona, phenomena that seem to indicate matter ejected from the Sun and returning to it are obviously of importance. The only other photograph in which I saw these curved rays is one on a large scale by Prof. Campbell, which was also taken near Jeur. I have not seen the Astronomer Royal's original negative. I do not say that these details are only visible in the two Jeur pictures, but if they were we should have evidence for a remarkable change in the corona in a short space of time.

Dr. Spitta. Does Mr. Wesley know the means adopted, so that he brings about the difference in exposure?

Mr. Wesley. So far as I understand, there is a shutter of a particular shape pivoted through the centre of a plate, which revolves. There was an opening of such a form that by rapidly turning the shutter the outer part gets a long exposure, and the inner part a shorter one.

Mr. Maures Horner. I do not understand about the curves. Do you think they are coronal curves, or do you think they have to do with the chromosphere?

Mr. Wesley. It is impossible to show the difference in white chalk, but these curves are very much fainter than those which are seen in the prominences. The curves are very much fainter and they are very much higher than any I have seen of prominences on the photographs. I feel sure they are coronal.

Mr. Maures Horner. With regard to the question of the chances of there being a rapid change while the observations were being made; whenever there is a very bright curving form it seems that a change should be expected. We may see a change in a very short space of time indeed, and it is possible there was a change.

Mr. Wesley. If there are real changes in the corona they are very important indeed. In 1893 there was practically no difference between the corona as seen in Chile, Brazil, and in West Africa

Mr. Newall. The photograph is rather important, as illustrating in a marked way the connection which the Astronomer Royal has traced between the coronal streamers and the prominences. It is especially interesting to me, for in my observations of coronium round the Sun there is a distinct bright patch of coronium between the two northernmost of the three prominences in the N.W. quadrant and not between the other two.

Mr. Maures Horner. In observing the coronium lines have you ever noticed that when you see a metallic prominence is in activity the line 1474 disappears? Before the prominence comes the usual line is seen, but afterwards with the utmost care it cannot be traced until the disturbance has ceased.

Mr. Newall. In my observations I was not using any slit. I was observing the corona with an objective grating, so that I had no means of judging of the appearance of the line as a line.

The President. The contrivance by which Mr. Burckhalter has produced these photographs seems to be extremely ingenious, and I am sure I am expressing your wish when I say that the Society is grateful to him for his communication.

Mr. Newall then read a paper by *Prof. E. E. Barnard* on observations of Hind's variable nebula in Taurus. Mr. Newall said that in 1895 Prof. Barnard sent the Society a communication with reference to this particular nebula, and this paper brought his observations up to date. The paper was so full of interest that it was impossible to give an abstract of it, and he therefore read it in full, the concluding part of the paper stating:—"In conclusion, we have evidence that Hind's nebula still exists, but it is seen very faintly even in the most powerful telescope under the very best conditions. It is possible that these clumps of nebulæ are due to slight fluctuation in light. The observations of 1895 show that though easily visible objects in February and March of that year they had thoroughly disappeared six or seven months later. It was brighter in 1895 than in 1890. Whether its visibility in 1890 was due to increased light when it was lost sight of many years before, or whether it was due to a more powerful telescope, we have no means of telling. Whether the light of the nebula is approaching extinction or is subject to fluctuation will be an interesting question to decide. One is impressed with the idea it had not been long of the brightness which existed when Hind found it in 1852. If so, we should be able to see it with a small telescope; but it must be watched with a large telescope, for it is of that class of nebulæ which are best seen through large telescopes."

Mr. Newall. From the apparent change in the position of the nebula with reference to T Tauri, it looks as if the observations can only be explained on the basis that there is considerable extended nebula about that region with a variability in different parts of the nebula, which is bright in some places and faint in others.

Prof. Herschel. Is this great T, or little T Tauri?

The President. Great T Tauri. We have too much reason to envy the Americans the climate, their telescopes, and, I may add, their men. Prof. Barnard has, I believe, recently been ill, and we may welcome this paper from him as a sign that he is restored to health.

Mr. Newall then read a paper by *Prof. W. Schur* on the determination of the diameter and compression of the planet Mars with the heliometer of the Göttingen Observatory. He said that the observations of Mars were made with the heliometer about the time of opposition on 1899, January 21, 23, and 26, and from these equatorial and polar diameters were deduced. The compression of the planet was determined from these observations, and Prof. Schur had combined his results of December 1896, which had already been communicated to the Society, with those taken in the present year. The observations taken in 1899 confirmed those taken in 1896, and the planet would therefore have a compression of about $\frac{1}{50}$. Professor Schur was of opinion that these results were to be trusted more than those of older times, as they had been made with a reversing prism, so that the peculiarity of the eyes of the observers, which gave different results of diameters of discs according as they were measured in different directions, was eliminated. The result of the heliometer observations was in contrast with those of H. Struve, his result being $\frac{1}{190}$.

Capt. Noble. I suppose that as these are differential and were all made with the same instrument, they may possess a certain amount of value, but they can scarcely be compared with measures made with the filar micrometer. No one gets the same result with the double-image micrometer and with the filar micrometer. They are differential and *pro tanto* perhaps they are valuable.

Mr. Newall. The only observations they are compared with are those by H. Struve.

Dr. Downing. Prof. J. C. Adams discussed the question of the compression of Mars from dynamical considerations, and, so far as my recollection goes, his result was rather in accordance with that of H. Struve. The result obtained by Prof. Schur is rather a startling one, but he has evidently taken great care, and his method is suited to an investigation of this kind, so that I think we must place considerable reliance on the result that he has obtained. I confess that at the moment I have no hypothesis to offer.

Mr. Crommelin. Does Prof. Schur make any correction for the difference of irradiation caused by the polar caps ?

Mr. Newall. That is a point in the paper that I ought to have referred to. Prof. Schur has considered that point.

Mr. Maw. Does he make reference to Barnard's measures ?

Mr. Newall. No.

Mr. Maw. These were mentioned in the Council Report of last year, and it would be interesting to have the note read.

The President. I cannot but find that Professor Schur's estimate of the compression of Mars is startlingly great. Phobos, the inner satellite of Mars, revolves in close proximity to the planet. Now, according to the researches of Edouard Roche, it is impossible that a satellite can revolve round its planet at less than a certain distance, without being rent apart by the intensity of tide-generating force. The radius of the circle, estimated as a multiple of the radius of the planet, is dependent to some extent on the relative densities of the two bodies. The mean distance of Phobos from Mars is 2·75 times the planet's radius, and it follows, by the application of Roche's law, that the satellite is more nearly equal in density to the planet than is the case with the Moon and Earth. This conclusion is in accordance with what we may suppose to be probable, and, as far as it goes, it tends to negative the assumption—necessary if Professor Schur's result is correct—that the law of density of Mars is entirely different from that of the Earth. If this great compression of the planet be true, we must postulate physical relationships which will permit the planet to be far denser at the surface than in the interior : and it would need much evidence to prove such a conclusion. Whilst observation must always possess incomparably greater weight than *a priori* reasoning, yet such reasoning may well cause us to scrutinize very closely such an important work as this. In any case, the Society owes its thanks to Professor Schur for the communication he has made.

Mr. Newall then read the note published in the 'Monthly Notices' of 1898 February, relating to Prof. Barnard's measures, which gave the compression of Mars as $\frac{1}{107}$.

Mr. Maunder then explained some lantern-slides which had been presented by Mr. Salmon, and the slides were shown on the screen. A small amount of flour and water was rubbed on a slip of microscope glass and then dried over a spirit-lamp ; the result was then photographed in the microscope, and, as the slides showed, reproduced many of the best known features of the lunar surface with surprising fidelity.

Mr. A. Stanley Williams having contributed a paper on the periodic variation in the colours of the equatorial belts of Jupiter, it was read by *Mr. Newall*, who said that Mr. Williams had collated the results published by various observers in an exhaustive manner from the year 1868 to the present time, with special reference to the change in the colour of the equatorial bands of Jupiter.

Mr. Thackeray then read a paper with reference to the Greenwich Meridian observations of Polaris 1836–93 with reference to personality, the constant of aberration, and the star's parallax.

Mr. Thackeray read a note, communicated by the Astronomer Royal, on the diurnal variations of the Nadir and Level of the Greenwich Transit-circle. For the last three and a half years it has been the practice to take observations of the nadir and the level about the time of the Sun observation in the morning, at the beginning of the evening watch, and at the end of the evening watch, to test any possible variation in the circle-reading of the nadir or of the level. These observations had been divided into three groups according to the period of 6 hours length in which they were made. A table was given comparing the circle readings made between 9^h and 15^h, and 21^h and 3^h, with those taken between 15^h and 21^h (about sunset). This comparison showed that the day observations and the night observations gave a result about $+0''\cdot17$ larger in both cases, so that there seemed to be a semi-diurnal variation of the nadir. A similar variation seemed to exist in the case of the level.

The President. In this matter of the variations of level I acknowledge myself to be somewhat of a heretic. Twenty years ago my brother Horace and I endeavoured to detect, by experiments carried on at the Cavendish Laboratory at Cambridge, the deflection of gravity produced by the Moon's attraction. Our instrument was supported on a block of stone weighing about a ton, and was of such delicacy that we were able to detect changes of level amounting only to a two-hundredth of a second of arc. We failed in the principal object of our inquiry; but we found —as many others have done—a large diurnal inequality in the level. This inequality was not, however, regularly periodic, and the normal path of the pendulum was often suspended, so that, for example, when we expected to find it moving northward it was moving southward. I do not suppose that the piers of a transit-circle are subject to inequalities of anything like the magnitude which we noted at Cambridge, but I am convinced that such inequalities exist, and that they are of a magnitude which is of importance in the most refined astronomical work. I think it likely that at some future time astronomers will make use of continuous methods of observing the level for the purpose of reducing these observations. Such a paper as the one which has just been read shows (what is, indeed, obvious) that astronomers are not oblivious of the importance of variations of level, and I venture, as one interested in the subject, to urge that such work as this be prosecuted with every possible care and energy. Our thanks are due to Mr. Thackeray and to the staff of Greenwich Observatory for the communication of this paper.

Mr. Thackeray. I am glad, Sir, to hear this expression of your opinion, that a more continuous record of these quantities should be made, for it coincides with the conclusion I drew from the result of this paper.

Mr. Newall then read a paper by *Mr. Denning* on the radiant-point of the April meteors (Lyrids). Mr. Denning dealt with observations dating from the year 1803; and gave details of a brilliant meteoric shower observed in Richmond, Virginia, and in several places in Massachusetts. One observer said there were too many meteors to be counted. Mr. Denning went on to deal with observations made in 1838 by Prof. Wright, in Tennessee, who counted 154 shooting-stars between 10 o'clock at night and 4 A.M. In 1887, two years later, his results supported those of 1885. He gave his own observations at Bristol of a number of Lyrids. These formed a very important contribution towards the solution of the question of the position of the radiant.

Prof. Herschel. I would caution anyone who attempts to verify the radiant-point position of the Lyrid meteor-stream as traced in Mr. Denning's paper, by watching the meteors of the shower next month, not to expect too close an agreement of the Lyrid meteors with these projected places; because the shower is both very capricious in intensity, and apparently also variable in its radiant-centre. In my own experience of its displays, the radiant-point in 1864 was at 277° +35°; and it was similarly marked, close to α Lyræ (278° +38½°), by Heis, Galle and Karlinski, and Mr. Greg, in 1867–74; but by a position noted by Serpieri in 1869, and by Mr. Denning's observations since 1873, the shower's centre seems to have come into better agreement now with the cometary position at 270° +33°. The shower in 1864 was the finest Lyrid display of our time, and its radiant at 277° +35° could not be very materially mistaken; but the farthest motion in Right Ascension allowed to the radiant-point, in Mr. Denning's paper, now seems to be from 263° to 275°; so that since 1864–1874 the shower's centre seems to have backed in R.A. considerably into its present good agreement with the cometary position. But the shower has for many years been very inconspicuous, and much mixed and diversified, apparently with companion or branch streams, so that positions of its centre have here and there been recorded at 268° +25°, 274° +26°, 265° +38°, 270° +40°, 282° +33°, and 280° +43°, covering an area quite 15° wide in Hercules and Lyra. An ephemeris of the radiant-point's progressive motion must be difficult to trace and verify among meteors of such an irregular and scanty system. But it will at least mark very usefully the period of the shower's duration, and will make it a very interesting object to watch from night to night, as accurately as possible, the position of this stream's radiant-point from any meteors of the Lyrid shower which may be visible.

The President. The paper by Mr. Denning has taken but a few minutes to read; but, in recording our thanks to Mr. Denning, I would ask you to reflect upon the hours, weeks, and years of faithful observation and careful record which he has bestowed upon this subject: we owe him a great debt of gratitude.

Mr. Newall. We have also received papers from the Cape of Good Hope on nebulæ observed during the year 1898; and one from *Mr. de Sitter* on the use of the electric light for the artificial star of a Zöllner photometer. Also a Report from Sydney Observatory, received too late to be included in the February number. You will be glad to hear that an arrangement has been made by which the measurements of the astrographic plates will be carried out jointly with those taken at Melbourne. From the Cape of Good Hope we have also a paper referring to the occultations observed during the lunar eclipse on 1898, Dec. 27.

The following papers were announced and partly read:—

Royal Observatory, Cape of Good Hope. "Occultations observed during the Lunar Eclipse of 1898, Dec. 27."

Royal Observatory, Cape of Good Hope. "Nebulæ observed during the Year 1898."

W. de Sitter. "On the Use of the Electric Light for the Artificial Star of a Zöllner Photometer."

W. F. Denning. "The Radiant-point of the April Meteors (Lyrids)."

E. E. Barnard. "Observations of Hind's Variable Nebula in Taurus."

Prof. W. Schur. "Determination of the Diameter and Compression of the Planet Mars with the Heliometer of the Göttingen Observatory."

A. Stanley Williams. "Periodic Variation in the Colours of the Equatorial Belts of Jupiter."

W. H. Maw. "Double-star Observations, 1895–98."

Royal Observatory, Greenwich. "Note on the Diurnal Variations of the Nadir and Level of the Greenwich Transit-circle."

W. G. Thackeray. "The Greenwich Meridian Observations of Polaris, 1836–93, with Reference to Personality, the Constant of Aberration, and the Star's Parallax."

The following gentleman was elected a Fellow of the Society:—

Col. Thomas D. Sewell, 29 Grosvenor Road, S.W.

The following Candidates were proposed for election as Fellows of the Society:—

Rev. Edward Lyon Berthon, M.A., St. Margaret's, Cupernham, Romsey, Hants (proposed by Sir H. Grubb).

Rev. Theodore Evelyn Reece Phillips, M.A., Hendford Vicarage, Yeovil, Somerset (proposed by W. F. Denning).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The fourth Ordinary Meeting of the current Session of the British Astronomical Association was held at Sion College on Wednesday, February 22, *Mr. W. H. Maw*, President, in the Chair.

The election of six new Members by the Council was confirmed, and the names of eight Candidates for Membership were read and suspended.

The President announced that, owing to a fixed engagement at Sion College, the Association would not be able to have the use of the hall on April 26. The Council had therefore decided to hold the April meeting a week earlier, viz. on Wednesday the 19th, and that it should take the form of a Conversazione, similar to the one held last year.

Mr. Crommelin read a paper on "The Weather Prospects for the Total Solar Eclipse of May 28th, 1900, in Spain, Portugal, and Algiers." He said the shadow-track ran through a populous country, where railways were numerous; there would therefore be every facility for the party to divide, and this should certainly be done, as the weather conditions were not sufficiently favourable at any single station to render success a moral certainty. As a result of a careful study of the weather statistics, the conclusions he drew were:—(1) That Algiers should certainly be occupied, on account of its low cloud ratio, its accessibility, and its excellent harbour; (2) the Portuguese stations had a high, but not prohibitive, cloud ratio, and the longer totality ($1^m\ 39^s$ as compared with $1^m\ 6^s$ at Algiers) and the great accessibility of this region would tempt many; (3) the region south of Madrid had a low cloud ratio, and several railway-lines cut the shadow-track, so that there would be no difficulty in transporting instruments; (4) the Alicante region seemed less clear than Central Spain, but clearer than Portugal. The Association should charter a special steamer, which might proceed to Santander and land a party there for the neighbourhood of Madrid; it could then go to Oporto and land a party for Ovar and the neighbourhood; then to Lisbon, Cadiz, Gibraltar, Alicante, and finally to Algiers. The steamer should remain at Algiers as a floating hotel for the party, and after the eclipse return by the same route, picking up the parties at the different points.

Mr. Chambers said he had been asked to offer a few observations with regard to certain practical matters in continuation of Mr. Crommelin's paper. He concluded that Mr. Crommelin had not given much attention to Spanish railway matters, or he would not have spoken in so sanguine a way of the numerous railways crossing the line of totality and consequent facilities of access. It was quite true that the central line of the eclipse would cross eight lines of railway, and within the limits of totality there would be about 12 crossings. It was almost true to say, however, that while there were railways in Spain there were no trains; by which he meant that on the main lines of railway there were seldom more than two trains a day, and their average speed appeared to be about 15 miles an hour. He suggested that advantage should be taken of the compulsory slow travelling to see some of the undoubted beauties and sights of Spain and Portugal. A careful consideration of the whole subject had convinced him of the superior advantages of steamboat over railway travelling for the expedition. It took 50 hours or more to go from London to Oporto *via* Paris by express train, of which there were two a week, and the return ticket cost £34; while by steamer the voyage from Southampton to Oporto took 60 hours, and the circular return ticket, allowing them to land at one port and to return from another, cost £12.

Mr. Maunder expressed his opinion that it would be more satisfactory and more convenient to charter a special steamer, if possible; but to do this it would be necessary for 120 or 150 persons to join the party. He urged that those desirous of going should send in their names as early as possible, which was emphasized by Dr. Downing.

In reply to inquiries the President said that members would be allowed to bring friends and that names should be sent in to Mr. Maunder.

Mr. Cruise said that he had recently lived for over a year in Murcia and his experience of the weather was quite contrary to the report made by Mr. Crommelin. The sky was clear on the greater number of days in the month, and in fact cloudy days were extremely rare. With regard to travelling arrangements, he agreed with Mr. Chambers that there was a great want of comfort, but, although the railways were very abominable, there was a very complete system of coach travelling, which would enable them to get over many difficulties which would otherwise be experienced. The coaches were very comfortable and went very nearly as fast as a train.

The President then called on the *Rev. J. M. Bacon* to give an address on "The Balloon as an Observatory."

Mr. Bacon said he was not quite sure that observers made due allowance for the effect of the first half-mile of our atmosphere, beginning at the bottom.

Describing a night ascent in autumn, he said that before the balloon ascended 700 ft. the air was getting very considerably warmer, and after that height he entered a warm-air bath; it seemed as though the balloon passed through layers or pools of warmer air. Getting out of the warm stratum they entered one comparatively colder, and then came into the warm again. This sort of thing went on for hours. Far into the small hours of the morning they were sitting in a balmy, genial atmosphere, looking down on a scene that really did not seem one of earth at all, and when they descended they found themselves on a bleak hillside, with the thermometer nearly down to freezing. He imagined that the hours they were up that night would correspond to probably the busiest hours of amateur astronomers with their telescopes, and the conclusion that must be drawn would seem to be that from the very moment of sunset, onwards to midnight, warm air was collecting, and collecting copiously, at least up to the height which they reached, which was half a mile and more; so that through these hours there must be, under ordinary circumstances with clear skies, warm ascending currents and compensating descending colder currents.

The address was illustrated by a large number of lantern-slides, many of them from snap-shots taken from the balloon. After describing in detail two or three of his balloon ascents, Mr. Bacon concluded by giving an interesting account of some acoustical investigations carried on by him at the Maplin Lighthouse, and described several occasions on which he had heard sounds at very great distances.

The President inquired whether sufficient steadiness could be obtained in a balloon to keep an instrument on the Sun or Moon.

Mr. Bacon replied that all the photographs he had taken were absolutely snap-shots, and it would have to be a snap-shot at the corona.

The President tendered the thanks of the Association to Mr. Bacon for his interesting address, and the meeting adjourned at 7 P.M.

ROYAL METEOROLOGICAL SOCIETY.

The Monthly Meeting of this Society was held on Wednesday evening, March 15th, at the Institution of Civil Engineers, Great George Street, Westminster, *Mr. F. C. Bayard, LL.M.*, President, in the Chair.

Mr. F. J. Brodie read a paper on "The prolonged Deficiency of Rain in 1897 and 1898." For several years past there has existed over England, and especially over the central and south-eastern parts of the country, a remarkable tendency in favour of dry weather. The dry weather dealt with in this paper consequently came at a most inopportune time, and its effects, which would in any case have been sufficiently evident, were greatly aggravated by the state of things existing so long previously. Mr. Brodie discussed the rainfall records at 80 stations distributed over the British Isles, for the 18 months, April 1897 to September 1898; these were divided into three periods of 6 months each. During the period April to September 1897 the rainfall was in excess of the average over practically the whole of Ireland, the greater part of Scotland, and the north-west and south-west of England and Wales; while in the north of Scotland, and the central and the whole of the eastern part of England, there was a deficiency of rain, in some parts amounting to between 60 and 70 per cent.

During the period October 1897 to March 1898, with the exception of the north-west of Scotland and England, the rainfall was below the average all over the British Isles, the deficiency over the midland and south-eastern parts of England being from 50 to 60 per cent. below the average. During the period April to September 1898, two of the six months were excessively dry, and in the southern parts of England at least two others had a deficiency of rainfall. Taking the period as a whole, the rainfall over the eastern, midland, and southern counties amounted to less than 80 per cent. of the average, and in the south-eastern counties to less than 60 per cent., the smallest proportion of all being 51 per cent., in London. From an examination of the Greenwich rainfall records since 1841, it appears evident that, for length and severity combined, the recent spell of dry weather was the most remarkable experienced there during that period.

A paper on the " Climate of Jersey," by the *Rev. H. W. Yorke, M.A.*, was read by the *Secretary.* The situation and geological formation of the island, together with the action of the tides, have a great local effect on the general character of the weather. The climate, as a whole, is bright, genial, and sunny.

The Spectrum of Mira Ceti.

ADEQUATELY to discuss the spectrum of the " wonderful " star would be a task not lightly to be undertaken. Some of the most recondite questions in astrophysics are involved in the interpretation of that vario-tinted band, and enquiries about them have hardly advanced beyond the tentative stage. We must wait, then, for surer insight; and there is every prospect that it will be afforded ere long. For, although we are still in the wood, glades and clearings are already semi-discernible. My present aim, accordingly, is merely to bring out a few salient points in recent observations. Those of Professor Campbell are especially noteworthy. They had the two-fold advantage of being made with a magnificent apparatus and at an exceptionally high maximum. Of the skill employed in making them it were superfluous to speak. In more ways than one they mark a beginning. They both suggest relations and establish facts.

The plates, exposed with the Mills spectrograph lately attached to the great Lick refractor, show only the region near Hγ in such fine detail as to be available for measures of precision. These were designed primarily for the determination of the star's radial movement, which proved to be one of recession at a speed of sixty-two kilometres ($38\frac{1}{2}$ miles) a second. And since there was all but identity between the results obtained in 1897 and 1898, this velocity may be regarded as constant. But a *caveat* has to be entered. It was derived exclusively from the *dark* lines in the spectrum. The *bright* lines told a different tale. Four—two of hydrogen and two ascribed to iron—were compared; and all showed, relatively to the dark lines, considerable displacement towards the violet. Their absolute displacement was indeed at all times towards the red, although to a variable extent. But its fluctuations depended so obviously upon causes intrinsic to the lines themselves that recourse for an explanation to changes in the rate of motion seemed superfluous, if not positively misleading. Professor Campbell expressly disclaims any such hypothesis, nor do the data collected by him supply a jot of evidence in its favour. So far as appears yet, Mira must be regarded as a solitary body; and it may safely be admitted, on the authority of absorption-rays *physically* in a normal state, that it is in course of withdrawal from the Earth at a uniform velocity of thirty-eight miles per second.

Early in October, 1898, the star reached 2·6 magnitude; and during the couple of months of its greatest brightness the hydrogen-bands Hγ and Hδ were perceived to be broken up each into three unequal components. The significance of this circumstance cannot easily be over-estimated. It has of late years become familiar as a symptom of disturbance in stellar spectra of other types; but in those of "long-period variables" it has heretofore been unknown, either through non-detection or through non-existence. The "intensity-curves" representing the tripled rays in the 'Astrophysical Journal' for January are charged with meaning. In studying them it is difficult to escape the conviction that a "Zeeman effect" is really in question. The polarization-test alone can decide; and we may hopefully anticipate its application by Professor Campbell at the maximum of next September. The triplicity of the lines—supposing it due to magnetic action—shows that we are looking *across*, not *along*, the lines of force.

The phenomenon, however, may not infallibly recur at all maxima. Indications are not wanting that their gradations of brilliancy involve spectral distinctions. Thus, Professor Campbell found the iron-lines at λ 4308 and λ 4376 bright, while on the Potsdam and Stonyhurst plates of 1896 and 1897 respectively, the first came out strongly dark, the second was either unrecorded or dark. And Dr. Vogel stated expressly that, apart from those of hydrogen, no bright lines were present. Now the maximum observed by him was a low one; the rise did not carry the star much above the fourth magnitude, and afforded a brightness, accordingly, less than one-third that attained in 1898. That of November 1897 was of intermediate character; yet no extra rays emerged, or at least emerged with anything like certainty, since Father Sidgreaves' query as to whether the absorption-bands were brilliantly edged should probably receive a negative reply. Only then at the most vivid phases of Mira are metallic reversals likely to take place; and it may be that the production of multiple hydrogen-lines is restricted to equally favourable occasions.

What has been learned through recent photographic work may be summarized in the following items of apparently trustworthy information:—

(1) Mira Ceti is retreating from the Sun at a constant speed of 62 kilometres a second.

(2) The bright lines in its spectrum are sensibly displaced relatively to the dark lines. The shift, which is always towards the violet, seems to increase as the lines fade.

(3) The hydrogen rays are broken up, near the time of maximum, each into three unequal components. They resume singleness with the waning of the star.

(4) Iron-lines, conspicuously dark at low maxima, are bright at high maxima.

The confirmation and development of these indications, and the prosecution of the untried paths of research they suggest, should engage the earnest attention of specialists, as likely to lead to further results of importance.

Professor Campbell was unable to perceive any visual trace of the red or the green line of hydrogen. Yet the two succeeding members of the series glowed with singular intensity. "If an exposure," he tells us, "of an hour was required for recording the dark-line spectrum, an exposure of two minutes, under the same conditions, would record the $H\gamma$ band." And the $H\delta$ band shone no less vividly. Father Sidgreaves, too, speaks of the "extraordinary brilliancy" of the same pair; "it is too great," he adds, "to be shown on a drawing, or to be safely expressed by a number representing relative intensity." The absence of the lower radiations constitutes, then, an anomaly of the most pronounced kind; and it is accentuated by the diverse character of the hydrogen-spectrum in nebulæ and in bright-line helium-stars, where F and C respectively predominate. The constant invisibility, on the other hand, of the fifth line of hydrogen ($H\epsilon$), not alone in Mira, but in all its congeners, finds a ready explanation in the eclipsing intervention of the massive calcium-band H. But no masking-hypothesis seems available in the case of C and F. Professor J. J. Thomson's remark, adverted to in this connection by Miss Maury, that they show a striking difference of brilliancy at the two electrodes, C outshining F at the positive terminal, and *vice versâ*, gives the sole hint up to the present intimated towards the solution of the problem. A. M. Clerke.

Early Appearances of the Lyrid Meteor-Shower.

A period of less than six months (beginning in the middle of July) includes the epochs of six out of the seven principal meteoric showers. Hence a special interest attaches to the shower of Lyrids which returns annually between those of the Quadrantids and Perseids.

Ten probable appearances of Lyrids up to the commencement of the nineteenth century (1803) have been collected by Prof. Newton. The dates of these correspond to A.D. 1850, April $19^{d}\cdot6$ to $21^{d}\cdot3$.

The phenomena which come first in the list referred to—those of B.C. 687, March 16, and B.C. 15, March 25—are described in the Chinese records. There is some doubt, however, as to whether these observations do not really relate to one body only.

A.D. 582, March 31.—Chasles says that people at Soissons saw blazing objects in the sky. A "shower of blood" fell at Paris.

1093.—The same authority states that "stars fell from the sky towards the west."

1094, April 10.—"At this period," in the words of Matthew Paris, the historian, "so many stars fell from heaven that they could not be counted." Another account says that this year "was very remarkable for the number and fashion of gliding stars, which seemed to dash together in manner of a conflict."

1095, April 10.—"Over almost all this land and for nearly the whole of the night, stars were seen falling from heaven in manifold ways, not one or two at a time, but so thickly that no man could count them" (Saxon Chronicle). Chasles remarks that the shower lasted from midnight to dawn.

1096, April 10.—"Almost all the stars ran like dust before the wind from cock-crowing to dawn."—CHASLES. "During many nights, stars were seen to rain down at intervals, but so thick and fast, that one would have said they were flakes from the celestial orbs."—DE MEZERAY.

1122, April 11.—"At the fourth watch of the night . . . innumerable stars were seen falling, and, as it were, raining down throughout the world" (Chronicle of the Monastery of Mont Cassin).

1123, April 10–11.—Chasles records the appearance of "innumerable" shooting-stars.

1803, April 19, 13^{h}–15^{h}.—Many observers in the United States watched this magnificent display. At Richmond (Virginia) "starry meteors seemed to fall from every point in the heavens in such numbers as to resemble a shower of sky-rockets." The heavens seemed to be all on fire from the abundance of lucid meteors, some of which appeared to almost touch the ground. One witness counted 167 in about fifteen minutes, and could not then number them all. At times the light was so great that a pin might be picked up from the ground. Another says that the nearest were as large as a barrel (!), and had tails from 12 to 20 feet in length. These observations were made at Raleigh (North Carolina); Portsmouth (New Hampshire), Schoharie Co. (New York), and Wilmington (Delaware), as well as at Richmond. Remarkable April showers have occurred on many dates subsequent to 1803, the last in Prof. Newton's table.

1838, April 20, 10^h–16^h.—154 meteors.—Prof. Wright and an assistant, Knoxville (Tennessee).

1839, April 18, 12^h–15^h.—58 meteors (18, 17, and 23 in the respective hours; the Moon set about 13^h) were counted by Herrick and Bradley at Newhaven (Connecticut). The times of flight were generally less than half a second. The radiant was about 273° +45°, a position which was confirmed by an observer at Geneva (New York). Thus it would appear that the observations of this year were of a contemporary shower of Draconids.

April 19, $12^h\ 15^m$–$13^h\ 15^m$.—19 meteors; the radiant could not be so well determined.—Herrick and three other observers. Watches were kept at the Western Reserve College, Hudson (Ohio), on the same nights, the hourly rate of meteors being about 12.

1841, April 18, $8^h\ 30^m$–$11^h\ 30^m$.—Another radiant emitting meteors contemporaneously with that in Lyra was first remarked on this occasion. Prof. Forshey at Vidalia (Louisiana) placed this about 198° −8° (in Virgo). In $2^h\ 15^m$ (45^m was lost in recording) 60 meteors were seen, all but 5 of which passed within 10° of this point. They were of a reddish colour, and generally of the third and inferior magnitudes.

April 19, 11^h–12^h.—13 meteors in the S.W. quadrant; $12^h\ 30^m$–13^h, 7 meteors in whole sky. No very definite radiant could be determined, but it appeared that the radiant region was east of the meridian, and about 70° or 80° in altitude.—E. C. Herrick and two other observers. This is probably an observation of Coronids, now well known as a cum-Lyrid shower.

1842, April 20, $10^h\ 20^m$–16^h.—Elaborate arrangements were now made by Herrick and four assistants. The Moon interfered until 15^h. Between $10^h\ 20^m$ and 11^h 7 meteors were recorded, while the numbers in the following hours were 13, 20, 22, 34, and 55 respectively, the grand total being 151. A few radiated from Virgo, but no general radiant could be fixed for the majority.

1843, April 20.—The horary number of shooting-stars was 12 to 15 for one observer.—E. C. Herrick. On the following night meteors were unusually frequent.

1847, April 19, 20.—"Many meteors."—Colla, Parma, Italy.

1848, April 23, 10^h–12^h.—"An unusual number of meteors."—Symonds, River Clyde, Scotland.

1849, April 19, 13^h–14^h.—54 meteors; no decided point of radiation.—E. C. Herrick and two other observers.

1850, April 20, 8^h–$10^h\ 30^m$.—An extraordinary display of shooting-stars was seen at many places in India.

1860.—The April meteors are said to have been observed about this year, by M. du Chaillu, in the equatorial parts of Africa, almost as brilliant and leaving streaks more enduring than those of the great November star-shower.—Prof. Kirkwood.

1861, April 19, $14^h\ 45^m$–16^h.—52 meteors; no definite point of radiation.—E. C. Herrick and another observer.

1863, April 20, 11^h–12^h.—Falling stars very plentiful; directed from Lyra.—A. Walker, Strathdon, Aberdeenshire.

For a few hours meteors were as frequent as in a moderately bright August shower.—W. H. Wood, Weston-super-Mare.

Owing chiefly to the efforts of the Luminous Meteor Committee of the British Association in England, and those of similar bodies in Austria and Italy, as well as to the greater attention of other observers, it was about this time (1863) that the radiant-points of meteor-showers became more generally determined, and hence the history of the Lyrids enters upon what may be termed the "modern" stage.

Herrick, writing in 1838, said "the cycle of the April shower seems to be, perhaps, 27 years." In 1841 he added: "It is, however, not to be supposed that the cycle remains constant through successive ages."

In 1867 Prof. Weiss pointed out the close agreement of the radiant-point of the Lyrids with that of Comet 1861 I. (about 271° +33°)*. This comet has a period of 415·4 years (Oppolzer).

In 1870 Prof. Kirkwood adopted a period of about 28⅓ years to fit in with the principal meteor-showers already described. His reductions are shown in the following table:—

		Years.		Years.
B.C. 687 to B.C. 15	=	672·000	=	24×28·000.
15 „ A.D. 582	=	597·000	=	21×28·429.
A.D. 582 to 1093·714 (between 1093 and 1096)	=	511·714	=	18×28·429.
1093·714 to 1122·143	=	28·429	=	1×28·429.
1123·143 „ 1803	=	680·857	=	24×28·429.
1803 „ 1860	=	57·000	=	2×28⅓ (approximately).

It may be mentioned, in conclusion, that such a period would have brought a maximum in 1888, but no observations appear to have been made in that year. The next maximum would be due about 1917.

W. E. Besley.

70 Vincent Square, London, S.W.

Recent Observations of Jupiter.

The observations of Jupiter that it has been possible to secure here in the early mornings during the past two or three months, show that some interesting changes have taken place since the the planet was observed in July last.

The *north temperate belt* has apparently disappeared. The seeing has been almost always poor, and possibly this is accountable for the failure to recover it, but at any rate the belt must now be exceedingly feeble.

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* The mean of 48 positions assigned to the radiant-point by many observers, and gathered from various sources by the writer, is 272°·3 +34°·0.

The *north tropical zone* is now the seat of considerable disturbance. A number of dark and light spots have been visible at or close to the N. edge of the north equatorial belt, including what are believed to be the three dark spots "A," "B," and "C" of last apparition. The velocity of the N. tropical current has very perceptibly diminished since last year, the approximate mean derived from the observations of ten spots being $9^h\ 55^m\ 33^s.3$, whereas Mr. Denning found last year from the three spots mentioned above a period of $9^h\ 55^m\ 26^s.3$.

The *north equatorial belt* is now broader and more pronounced than during last apparition.

Like the N. tropical current the *great equatorial current* also appears to have slowed down, though to a somewhat less extent. The rate of rotation varies considerably in different longitudes, but from the mean of sixteen spots there is found an approximate period of $9^h\ 50^m\ 27^s.9$ as against $9^h\ 50^m\ 24^s.2$ last year.

On the other hand, the few and somewhat unsatisfactory observations that have been made here of the "hollow" or "bay" in the S. equatorial belt in which the red spot lies seem to show that the famous spot has somewhat increased its rotational velocity, and this conclusion is supported by observations kindly forwarded by other observers. Only a few vague traces of the spot itself have been usually visible in the poor seeing experienced this year, but the "hollow" which guides the eye to its position retains its old definiteness of outline.

Both the equatorial belts appear to be a dull chocolate-red in colour, with the equatorial zone between them a yellowish orange. The colours are always much more apparent when the planet is observed in twilight.

Hendford Vicarage, Yeovil,
1899, March 20.

T. E. R. Phillips.

A ninth Satellite to Saturn.

The latest sensation in astronomy was produced by a telegram forwarded from America through the Kiel Bureau on March 18, announcing that Prof. W. H. Pickering had discovered a ninth satellite to Saturn. The first brief announcement stated that it was seen on four photographs, and that its period was estimated at about seventeen months. A fuller telegram published in a morning paper, purporting to be on the authority of Prof. E. C. Pickering, stated that the plates were taken on August 16, 17, and 18 last, at Arequipa, with the Bruce telescope of 24 inches aperture and short focal length, and that the new object is estimated to be of magnitude rather fainter than Hyperion. It seems that, on comparing two plates taken on August 16 and August 18, a faint grey point was found on each, but neither point had a counterpart on the other plate relative to the stars, though each was in the neighbourhood of Saturn. A similar point was found on the plates taken on the intermediate day. The fact that the object had moved with Saturn was held to be conclusive evidence that it is a satellite. The suggestion that it might be an asteroid was considered untenable. From the data it seems that its period is eighteen months, and the distance from the primary about 8,000,000 miles.

These are the facts as known at present. Confirmatory observations of this addition to the solar system will be waited for with interest.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Bearing of Theory upon the observed Polar Motion.

Gentlemen,—

I was very glad to find the reproduction of Prof. Newcomb's interesting article in the 'Observatory' for March (p. 115), because, when I originally read it, it gave rise to some questions that I desired to ask, but which I could not state in the 'Astronomical Journal,' for the reason that the Editor of that periodical ruled the communication out peremptorily, on the ground that its space was needed for other contributors. I trust that the 'Observatory' will be more catholic.

One of the most tangible conclusions drawn in Prof. Newcomb's article is the following:—"It is extremely improbable, one might say it is incompatible with any probable law of change in the atmospheric movements, that the annual revolution should be in a markedly elliptic path. That is to say, Mr. Chandler's conclusion that the annual term is a very elongated ellipse, is not easily explicable by the probable action of any known cause." This inference is a highly important and interesting one, and is quite positive in its form of statement. Nevertheless it is by no means a self-evident proposition, nor does it seem to me that the argument leading up to it has the distinctness and force which he apparently attaches to it. I am sure he will pardon me for giving my reasons for regarding it as open to doubt. In order that the reader who has not given special attention to the subject may apprehend clearly the real point where difference of opinion arises, and where the rub of argument must come, a few lines of preliminary statement are necessary.

Let the co-ordinates of the pole of figure, P, in its annual motion about a mean position, P_0, be

$$x = a\cos(mt+C), \quad y = b\sin(mt+C),$$

and those of the pole of rotation, R, be X and Y. Since the present condition of theory requires and permits it to be assumed that R moves always at a right angle to the line RP, with the constant angular velocity μ, the differential equations of the motion are:—

$$(1) \qquad dX = -\mu(Y-y)dt, \quad dY = +\mu(X-x)dt.$$

Take, for the co-ordinates of the motion of R about P_0, the general forms

$$(2) \qquad X = a\cos(mt+C)+r\cos\mu t, \quad Y = \beta\sin(mt+C)+r\sin\mu t.$$

Substituting the differentials and the co-ordinates in (1) as required, we derive

$$(3) \qquad a = \alpha-\beta\frac{m}{\mu}, \quad b = \beta-\alpha\frac{m}{\mu}.$$

Now, from the observations, I have determined the axes of the annual ellipse to be 0''·275 and 0''·085. Therefore

$$\alpha = 0''\cdot137, \quad \beta = 0''\cdot042\,;$$

also we have

$$\frac{m}{\mu} = \frac{0^{\circ}\cdot985}{0^{\circ}\cdot843} = \frac{7}{6}\cdot$$

Inserting these in (3) we get

$$a = +0''\cdot09, \quad b = -0''\cdot12.$$

That is to say, an annual movement of the axis of inertia in a retrograde direction about a mean point, in an ellipse 0''·18 by 0''·24, will, if the theory be correct, give a resultant annual direct motion (west to east) of the rotation-axis in the very elongated ellipse 0''·275 by 0''·085, as actually observed. The major axes of the two ellipses lie at right angles.

Prof. Newcomb's conclusion, therefore, seems to be equivalent to saying either: first, that the perturbation of the pole of figure must be so near an exact circle that even so moderate a deviation from circularity as is implied in a flattening of only one-quarter is incompatible with or inexplicable by the probable action of change in water or air currents, or any known cause; or, second, that these causes cannot, with any probability, give rise to a periodical displacement of the axis of inertia with an average radius of 0''·10, which is all that theory requires, as above shown, to produce the actually observed annual term of 0''·275 by 0''·085. I am not quite sure on which of these considerations, namely whether on shape or size, he rests his view of improbability and incompatibility. But it does not appear to me that our knowledge of the facts pertaining to the causes in question permits any such nice discrimination, or anything but mere guesswork, as to either of these points. Nor am I able to see how or where, in Prof. Newcomb's papers, he has established the connection between these facts and the conclusion he draws which I have quoted in the beginning of this note, with sufficient distinctness to enable me to appreciate its force. Since there may be other readers equally dull, a few words of enlightenment would be helpful. It is important that a contradiction between theory and observation in so important a question as this should, if possible, be clearly made out.

With regard to Prof. Newcomb's concluding remark, it seems pertinent, though trite, to add that, in any case involving a *demonstrated* conflict of an accepted theory with an *established* result of observation, it is the theory which must ultimately give way. In the case in hand I am merely raising the question whether such a conflict has been reasonably demonstrated, as the first one to settle. If that is made certain or highly probable, then, if the fact of the nearly rectilinear annual component of the polar motion is beyond peradventure, we must naturally revert to a distrust of the accepted theoretical law of motion embodied in the differential equations (1) above. I confess to very little confidence in the latter, for my own part; my doubts resting on a contradiction, by certain facts of observation, of a very different sort from the one suggested by Prof. Newcomb. Into this, however, it is impossible to go in this note, which is already much longer than was intended.

S. C. CHANDLER.

Boston, 1899 March.

Spectroscopic Observation of Velocity.

GENTLEMEN,—

On page 461 of the December number of the 'Observatory' there is a small error, which I may perhaps be pardoned for pointing out, since it relates to a subject of very considerable interest—the degree of precision which is now attainable in spectroscopic measurements of motion in the line of sight.

One would, I think, infer from the paragraph above referred to that the values for the motion of the planetary nebula G.C. 4373 (−80·9 and −64·7 km. per sec.) were independently obtained by two different persons; whereas the former value is merely the latter corrected for the assumed motion of the solar system.

Considering the difficulty of these observations, a difference of even 14 km. between the results of independent observers might be regarded as indicating a satisfactory degree of precision. In fact, however, the results of independent measures, where such have been made, agree somewhat more closely than this. The motions of a few of the nebulæ considered in my memoir ('Publications of the Lick Observatory,' vol. iii.) have been remeasured by Professor Campbell, whose results in no case differ by more than

2 miles, or about 3 km., from mine. For the fainter nebulæ the agreement would doubtless be less satisfactory.

In our systematic work on stars by the photographic method, to which a large part of the time of the 36-inch equatorial is devoted, the accuracy attained is much greater, the probable error of a single determination being only a fraction of a kilometre per second. This high degree of precision is due to the larger aperture of our telescope, to favourable meteorological conditions (transparent sky, good seeing, and constant temperature), to an efficient and rigid form of spectroscope, and to numerous precautions in observing, for an account of which I must refer to Professor Campbell's article in the October number of the 'Astrophysical Journal.'

Lick Observatory, California. JAMES E. KEELER.
1899, March 1.

[Our thanks are due to Prof. Keeler for pointing out our oversight, which, however, we can scarcely regret, as it has been the cause of his writing the above interesting letter.—EDS.]

The Causes of the Gegenschein.

GENTLEMEN,—

Mr. Anderson may be right (page 94 in February) in suggesting that the Gegenschein and the Zodiacal band are distinct objects; but is it not much more simple to suppose that they are the same, the latter being the outer extension of the Zodiacal Light and the former a merely optical appearance thereon? In this case, it is true that theoretically there ought to be the shadow of the Earth in its centre; but if, as is probable, the Zodiacal Light extends tens if not hundreds of millions of miles beyond the Earth, it is not likely there would be a trace of the shadow perceptible. Mr. Evershed's theory that the Gegenschein is a tail to the Earth, is not impossible; but the average of the whole of my observations up to 1895, Sept. 25 (which I propose to publish in the forthcoming publications of this observatory), place the centre only 0°·6 preceding and 0°·75 north of the anti-Sun; the latter deviation from the anti-Sun has been explained by A. Searle as due to atmospheric absorption.

West Hendon House, Sunderland, Yours faithfully,
1899, March 20. T. W. BACKHOUSE.

Copernicus and Regiomontanus.

GENTLEMEN,—

I had occasion, a few years ago, to point out (see 'Notes and Queries,' 7th Series, viii. p. 84) a remarkable error in the last edition of the 'Encyclopædia Britannica' (vol. ix. p. 346), where we are told that Copernicus, in the year 1500, "was at Rome, enjoying the friendship of the astronomer Regiomontanus." This is a little modified from the statement in the preceding (eighth) edition, which is that Copernicus went to Rome for the express purpose of visiting Regiomontanus, "who kindly received him there." As a matter of fact, Regiomontanus (Müller) died at Rome in 1476, when Copernicus was only three years old, and still at his birthplace, Thorn. But in Poggendorff's 'Biographisch-Literarisches Handwörterbuch' a still more remarkable statement is made about him, that he studied astronomy first at Cracow, "dann zu Wien unter Peuerbach und Regiomontan." Now Peuerbach (or Purbach) died at Vienna in 1461, twelve years before the birth of Copernicus, and it does not appear that the latter ever resided at Vienna at all, though he probably passed through it on his way to Italy. A small monograph on Regiomontanus was published by Alexander Ziegler at Dresden in 1874. In the title of this work he calls Müller "ein geistiger Vorläufer des Columbus," because the ephemerides of the former were of great use to the latter in his voyages, and therefore through them, Ziegler argues, Germany contributed to the geographical discoveries of that age. That Columbus was in possession of these is proved by the fact that it was by their means that he predicted to the natives of Jamaica the eclipse of the Moon which occurred on the 29th of February, 1504, and thus frightened them into supplying him with the provisions of which he was much in need. Those ephemerides extended from 1475 to 1506, and that they were used for the above purpose is stated in a fragment of Columbus's journal, preserved in the work of Navarrete. Moreover, Oscar Peschel informs us, in his 'Geschichte des Zeitalters der Entdeckungen,' that Columbus approximately determined his geographical longitude by these ephemerides, and predicted the lunar eclipses of the 14th September, 1494, and the above-mentioned of the 29th February, 1504.

Müller, as is well known, took the name of Regiomontanus from the Latinized form of that of his birthplace, Königsberg in Lower Franconia, where he was born on 6th June, 1436. When twelve years old he proceeded to Leipzig to study at the University there, and three years afterwards, in 1451, removed to Vienna and became a pupil of Purbach (then in his twenty-eighth year), whose real name not being known, he is always called by that which he took from his birthplace, Purbach, which is on the Emms in Austria. After the death of the latter in 1461, Regiomontanus went to Rome with Cardinal Bessarion, who took an active part in the revival of the study of Greek literature in Western Europe. He remained in Italy until 1468, and then returned to Vienna, but soon afterwards accepted an invitation from Matthias, king of Hungary, to stay at Ofen. It was in 1471 that he took up his abode at Nürnberg, where Bernhard Walther had erected an observatory, in which they worked together until, in 1475, Regiomontanus was summoned to Rome by the Pope to assist in the projected reformation of the Calendar; but he died there in the following year, which was the third after the birth of Copernicus.

Yours faithfully,

Blackheath, 1899, March 1. W. T. LYNN.

OBSERVATORIES.

WASHINGTON (U.S. NAVAL).—Prof. Harkness has kindly sent us a copy of his Report for the twelve months ending 1898, June 30, concerning which it is almost enough to say that the energetic employment of the individual instruments as recorded in the last Report* has been continued. The equatorials have been used as before for making micrometer measures of planets and satellites—41 complete measurements of the satellite of Neptune having been obtained with the 26-inch—and for determining positions of comets and small planets. The Observatory has had two new instruments added to its equipment: a 6-inch transit-circle, the tube of which consists of two halves turned out of solid steel bars bolted to a cube which was machined with great accuracy, the idea being to reduce the effects of flexure. This instrument is reversible. We shall look for results with this with some interest. The other new instrument is a 5-inch altazimuth, also made of steel, in fact it is constructed on much the same principles as the transit-circle. Both instruments were made by the firm of Warner and Swasey. The work of the Department of the Nautical Almanac, which is also under the direction of Prof. Harkness, is reported on in the same pages, and from this we see that we may soon expect Prof. Newcomb's Catalogue of Fundamental Stars for the Epochs 1875 and 1890, which he undertook at the request of the Paris Conference of May 1896.

PUBLICATIONS.

RECENT ADVANCES IN ASTRONOMY †.—In the Publishers' Preface we remark the statement that "The individual volumes are contributed by leading specialists in the various branches of knowledge which fall to be treated in the series." But, in the case of the particular volume that we have now before us, this statement is entirely misleading, for the branch of science in which its author, Dr. Fison, is a leading specialist is not astronomy, and we do not know of any special contributions to its advancement that he has made, or that he is actively engaged in astronomical pursuits, either practical or theoretical, otherwise than those of a University Extension Lecturer.

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* 'Observatory,' 1898 February.

† By Alfred H. Fison, D.Sc. The Victorian Era Series. London: Blackie & Son, Limited, 50 Old Bailey, E.C., 1898.

Nevertheless, though Dr. Fison speaks as one of the scribes and not as it were with authority, the little volume that he has now issued is written with some life and vigour and will prove useful to the student in those departments with which it deals.

The title of the book is far too comprehensive, for Dr. Fison has very wisely, we think, limited his treatment to but a very few of the recent advances in astronomy. The small volume contains but 237 pages, a space manifestly inadequate even to index the manners and methods of astronomical progress within the Victorian era. Even with his limitation to the three sections of the life and distribution of stars, of Mars, and of spectroscopy, his treatment of each is for the most part of the baldest.

In the first chapter the parallax of stars leads on directly to their distance from us and to their nature as "suns," to their relation to nebulæ, their variability and the presence of dimmer and of dark suns; of the presence of "stellar systems" more complicated than, but of the same kind as, our solar system, of the maintenance of heat in a sun, and of the stellar motions; leading finally to the very pessimistic prophecy that "there appears, so far as physical science has interpreted the processes of nature, no escape from the picture of an accumulation of inert matter as the last memorial of a glorious universe of suns."

This chapter is the most fully, clearly, and accurately written. When treating so largely of the parallaxes of stars we would suggest that Dr. Fison should have entered into the subject more fully still, and have not omitted the practical details of the methods. It cannot be denied that photography offers a valuable weapon of research; and though photographic parallax hunters seem at present engaged—like Joseph's brethren—in falling out by the way, we may hope that this is but a temporary phase of the enquiry, and that much advance may be made in this department, since it lies within the photographic power of some amateur astronomers, as well as of public observatories.

A sin of commission is the inclusion of Mädler's absolutely baseless hypothesis of a central controlling power in the Pleiades. This fiction has in this way become almost a religious dogma in the popular mind, and is even assumed as so fundamentally true that theological tenets can be founded on it.

In the chapter dealing with the Milky Way and star-distribution, Dr. Fison again assumes the rôle of a prophet. He ingeniously supposes a condition of things in the starry spaces not unlike that imagined in the kinetic theory of gases. He concludes that some of the star masses may attain, by their encounters, velocities without limit as to magnitude and such as are incapable of generation by gravitational attraction. Therefore he says "it is conceivable that in the remote past the sun cluster may have been far richer than it is now, and the firmament may have been more resplendent with brilliant stars, but that from age to age its members may have been gradually scattered, and the vault of heaven may now be growing poorer." Is there not some inconsistency between this prediction and the other to which we alluded above?

His study of Mars is exceedingly good and shows that it is written by one skilled in working with delicate and elusive objects, and who can weigh the amount of evidence that should be given to any observation, no matter how backed by positive assertion.

The spectroscopic studies are sadly insufficient. Dr. Fison scarcely seems to have got beyond the laboratory portion of this subject; and he brings his study of the Sun down no further than the year 1868, when its serious and systematic history may be said to have begun. Since Dr. Fison was so greatly limited in his space, we think it would have been better to have omitted this portion than to have treated it so inadequately, and have devoted the pages thus saved to the other subjects of the book.

A. S. D. M.

DANTE'S ASTRONOMY *.—In his work 'Sulla Data del Viaggio Dantesco,' published in 1897, Prof. Angelitti made out a strong case for his theory that the date of Dante's imaginary journey through the three worlds was the year 1301. His reply to the criticisms of Marzi not only maintains but strengthens his position. To the greater number of Dante lovers, who read him only for the beauty of his poetry and his thoughts, it may seem of infinitely small moment whether he imagined his vision to have taken place in March or April, or in one year rather than another; and many pass over the astronomical allusions, fancying that Dante's science, being mediæval, must all be erroneous. Yet a professor of astronomy, and director of a modern observatory has, by a careful comparison of passages in the Divine Comedy with the actual positions of the heavenly bodies at the disputed dates, not only supplied a very valuable contribution to the inner circle of Dante experts, but has reached a result which should be of interest to the larger circle also. They cannot fail to sympathize with Prof. Angelitti in his delight when, after each long and laborious calculation, he finds that the poet was as accurate in his science as he is exquisite in his verse. With no other artist would one think of applying such a test; but when it is shown that on Palm Sunday, March 25, 1301, the Moon was full, the Sun in the first degrees of Aries, Mars in Leo, Venus a morning star in Pisces, and Saturn "on the Lion's breast," as is required by Dante's descriptions, while in April of the same year, or in any month of the years just preceding or following, these conditions were not all fulfilled, there is certainly a strong presumption in favour of the former. Whether the poet consulted the calendars, or whether, as our author thinks, he had himself looked upon "the

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* 'Sull' Anno della Visione Dantesca: Nuove Considerazioni in Replica a una Critica di Demetrio Marzi.' By F. Angelitti. Naples, 1898.

beautiful things that heaven holds," and had noted the significant aspect of the planets, there can be little doubt that his imagination here, as always, is as precise as vivid, and that if he says he saw Venus at dawn from the mount of Purgatory, Venus was thus visible.

From one difficulty in the way of the 1301 theory—the allusion to "questo centesim' anno," Par. ix. 40—Prof. Angelitti's critic has himself suggested a plausible escape. A more serious one is Dante's habit of applying the zodiacal names indifferently to the signs or the constellations, but in the descriptive passages under consideration it is clear which is intended. We heartily congratulate Prof. Angelitti on his work, and trust that others will supplement it by renewed examination and comparison of the historical and other allusions bearing on the question of date.

M. A. O.

PUBLICAZIONI DELLA SPECOLA VATICANA, VOL. V.—The fourth volume of the publications of the Papal Observatory, containing the observations of the year 1893, was issued in 1895. The appearance of the fifth gives us the opportunity of reporting on work done at this institution in the years 1894 and 1895.

The volume, which is published by Signor Lais, the Vice-Director, opens with an obituary notice of Father Denza, who died on 1894 December 14, and continues with sections on astronomy, astrography, solar observations, magnetism, meteorology, and seismology. The astronomical work comprises observations of two comets and of meteors and a determination of the orbit of the minor planet Vaticana (416). The only meridian work done appears to be transits of the Sun for determination of clock-error. The results of the observations of sun-spots at this observatory are used each year by Prof. Wolfer to determine his "sun-spot numbers," and these are repeated in the volume. It may be remembered that the Vatican Observatory is taking part in the formation of the Astrographic Chart, and is responsible for the zone +55° to +65°. This work seems to have been hindered by some difficulties; a number of plates, first taken for the Catalogue series, were for some reason considered not satisfactory and have been replaced by a second series. It is stated that 71 plates have since been taken and passed for the Catalogue, and 92 for the Chart, but it is not quite clear to what date this refers. The taking of Chart plates is now suspended, and there is no mention of measurement. There are some very beautiful specimens of photographs of nebulæ given at the end of the book.

A large portion of the volume is devoted to magnetism and meteorology, and to reports of weekly meetings held at the Specola Vaticana, which give a fairly complete history of the science during the year.

A LABORATORY MANUAL IN ASTRONOMY*.—That delightful story about the advanced mathematician who tried to calculate the time of sunrise, and produced the answer in *square miles*, has already been told in our Oxford Note-Book, but it will bear repetition and is not without an astronomical analogy. It would not be hard to find persons who could do a very creditable examination paper in astronomy, but who would be hard put to it to at once point out the position of the ecliptic on a starry night or by rapid inspection to say whether the Moon were waxing or waning. Miss Mary Byrd, the Director of the Smith College Observatory, has written a book of quite a novel kind, to teach such elementary astronomy. The book, in fact, consists mainly of little problems based on data found in almanacs and maps and on results found from naked-eye observations. From the many examples it is difficult to select those most illustrative, but the following may be taken as types. On Feb. 4 the Sun's meridian altitude was found by means of the shadow of a tree, the time at rising and setting and its azimuth were observed on the same day by other terrestrial marks, and similar observations were made on other days of the year. With these as data the Sun's daily and annual path is evolved. Here is another investigation: three observations of the *shape* of the lunar terminator were made on consecutive nights, 1896 Dec. 10, Dec. 11, Dec. 12, to determine the time of first quarter, and similar observations on the 9th and 10th of the following February. From these the synodic period of the Moon was deduced as $29^{d}\ 21^{h}\cdot 7$. The length of the period given in the ephemeris was $29^{d}\ 21^{h}\cdot 5$. We should think Miss Byrd's class must find their work extremely interesting, and we can confidently recommend the book not only to *all* teachers of astronomy, but also to that larger class of persons who take an interest in astronomy but do not make any attempt at observations because they think it necessitates an expensive instrument. There are several appendices, of which it will be sufficient description to give the title:—"A home-made Telescope," by Dr. G. Pyburn; "Suggestions to Observers of the Zodiacal Light," by Prof. A. Searle; "A short Method of finding the Time when the Moon rises and sets," by Prof. E. Frisby; and three others which are tabular.

ASTRONOMICAL OBSERVATIONS AND RESEARCHES MADE AT DUNSINK.—The volume just received contains the separate results of 4022 observations of right ascension and 3999 of declination made during the years 1896 and 1897, together with a catalogue of mean places for 1900. The catalogue contains 1101 stars. As the observations were made, and in great part reduced and passed for press, during Dr. Rambaut's tenure of office, the volume is published under his superintendence. A feature in the catalogue, and one now too often left out, is the space devoted to references to standard catalogues where each star may be found.

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* By Mary E. Byrd, A.B., Director of the Observatory, Smith College. Ginn & Co., Boston, 1899. Price $1·35.

NOTES.

Comet Notes.—A bright comet, visible to the naked eye, with a short tail, was discovered by Prof. Lewis Swift at the Lowe Observatory on March 3. Its position was unfavourable for observation in these latitudes, and it is now out of reach in the neighbourhood of the Sun. But at the beginning of May it will be observable in the morning sky, and it will quickly reach a very favourable position for observation. The brightness on May 1 is 1·8, and on May 16 1·4 (that at discovery being unity), so that the comet will probably be visible to the naked eye. The following elements are by Dr. Kreutz from observations on March 4, 6, 9 (Ast. Nach. 3553):—

T	1899 April 13·03 G.M.T.	
ω	8° 26′	1899·0.
☊	24 46	
i	146 15	
$\log q$	9·5148	

Ephemeris for Greenwich midnight, by Dr. Smart.

	R.A.	N. Dec.		R.A.	N. Dec.
	h m	° ′		h m	° ′
May 2	0 5	23 30	May 14	23 8	37 23
6	23 48	27 30	18	22 38	43 28
10	23 30	32 13			

Tuttle's periodic comet was detected on its return by Dr. Wolf at Heidelberg on March 5, being of 11½ magnitude. In spite of its faintness, numerous observations have been obtained.

The following approximate elements and ephemeris are by J. Rahts (Ast. Nach 3552, corrected):—

T	1899 May 14·0 Berlin M.T.
π	116° 29′ 3″
☊	269 49 54
i	54 29 16
ϕ	55 15 24
μ	259″·623
$\log a$	0·757109

Ephemeris for Berlin midnight.

	R.A.	N. Dec.		R.A.	N. Dec.
	h m s	° ′		h m s	° ′
Apr. 2	3 3 1	23 51	Apr. 14	3 47 11	19 11
6	3 17 51	22 23	18	4 1 34	17 27
10	3 32 35	20 50	22	4 15 47	15 38

Ast. Nach. 3553 contains the following search ephemeris of Holmes's Comet, for Greenwich noon, by H. J. Zwiers.

He concludes that the perihelion passage takes place 1899 April 27·6651 G.M.T. The brightness during the period of the ephemeris will increase from what it was in Feb. 1893 to what it was early in Dec. 1892.

	R.A. h m	N. Dec. °		R.A. h m	N. Dec. °
May 2	0 7	5·2	June 19	1 29	20·0
10	0 21	7·7	27	1 42	22·4
18	0 35	10·2	July 5	1 55	24·7
26	0 49	12·7	13	2 7	27·0
June 3	1 2	15·1	21	2 19	29·3
11	1 16	17·6	29	2 30	31·5

The error will probably not exceed 8^m in R.A., 1° in Dec.

Ast. Nach. 3550–51 contains definitive elements by R. Aitken of Comet 1896 III., which was discovered by Swift on 1896 April 16, and observed till June 20. The number of observations employed is very large, and he deduces the following hyperbolic elements:—

			Probable error.
T	1896 April 17·64731 G.M.T.		0·00057
π	179° 59′ 15″	1896·0.	4″
☊	178 14 51		7
i	55 34 25		9
q	0·566286		0·000013
e	1·00048		0·00010

Ast. Journ. 456 contains the following elliptical elements of Comet 1898 I. (Perrine), deduced by Heber D. Curtis from 24 observations extending from March 20 to July 20, 1898:—

Epoch	1898 March 20·0 G.M.T.	
M........	0° 0′ 25″	
ω	47 18 20	1898·0.
☊........	262 26 4	
i	72 31 56	
ϕ	78 29 56	
log a	1·73672	
log q	0·03946	
log e	9·99119	
log μ	0·94493	

Perihelion Passage 1898 March 17·1189 G.M.T.
Period = 402·8 years.

Ast. Nach. 3549 contains the following ephemeris for Berlin midnight of Comet 1898 X. (Brooks) by F. Ristenpart. The comet may still be seen in the southern hemisphere, though only of the 12th magnitude:—

	R.A.	S. Dec.		R.A.	S. Dec.
	h m s	° '		h m s	° '
Apr. 27 ..	13 22 7	64 3	May 9....	12 24 54	58 50
May 1 ..	12 59 24	62 29	13....	12 12 22	56 55
5 ..	12 40 26	60 43	17....	12 2 19	55 1

A. C. D. C.

MINOR PLANET NOTES.—Three new planets have been discovered during the month, as follows:—

	Date of discovery.	Discoverer.	Place.
EG......	March 2	Wolf—Schwassmann.	Heidelberg.
EI	" 3	" "	"
EK......	" 9	Palisa.	Vienna.

The planet provisionally designated EH proved to be identical with (224) Oceana.

We continue the ephemeris for (433) Eros, as this planet is still observable, being of magnitude 12·4. The ephemeris is by J. Möller for Berlin midnight (Ast. Nach. 3552):—

	R.A.	N. Dec.		R.A.	N. Dec.
	h m s	° '		h m s	° '
Apr. 1....	3 26 36	22 59	Apr. 17....	4 28 53	24 11
5....	3 41 53	23 25	21....	4 44 54	24 16
9....	3 57 22	23 40	25....	5 1 6	24 14
13....	4 13 3	24 1	29....	5 17 29	24 8

A. C. D. C.

OBITUARY.—Miss E. BROWN. The news of the death of Miss Elizabeth Brown on the evening of Sunday, March 5, came as a very great shock to her numerous friends. For several years past Miss Brown has been a leading figure in amateur astronomy, and to her special department—the drawing and description of solar spots and faculæ—she devoted a large part of the last years of her life. She contributed her first paper to the Liverpool Astronomical Society, on the "Proper Motion of Sun-spots," on 1883, December 17, and was asked to become Director of the Solar Section of that Society very shortly after its inception. After the dissolution of the Liverpool Society she was very anxious that an astronomical association on similar but wider and firmer lines should be formed, and when the British Astronomical Association, in 1890, filled this want, she became Director of its Solar Section, and an ardent worker not only in her own but in several other of the sections, notably in those for the observation of the Moon, of coloured and of variable stars. She was a frequent attendant at the meetings of the Royal Astronomical Society. She was a Fellow of the Royal Meteorological Society, and one of her observatories at Cirencester was devoted to meteorological instruments, where she continued the observations of her father, the late Mr. Thos. C. Brown. Though by no means robust, she went on three long and arduous eclipse expeditions—to Russia, to Trinidad, and to Lapland. On only one of the three was she favoured by a clear sky; but the fruitless expedition to Russia in 1887 and the fruitful one to Trinidad in 1889 were commemorated by two beautifully and vividly written narratives—'In Pursuit of a Shadow' and 'Caught in the Tropics.' She was even now making preparations to go on a fourth eclipse expedition, to Spain or Africa. Those of her friends who are looking forward to the same expedition will no doubt feel that it has lost much of its pleasure by her absence. A. S. D. M.

SIR HOWARD GRUBB ON THE PARIS EXHIBITION TELESCOPE.—At a recent meeting of the Royal Dublin Society Sir Howard Grubb read a short note on the telescope which is said to be now being made by the firm of Gautier, as a show for the Paris Exhibition of next year. By kind permission of Sir Howard we are able to give the following abstract:—

The Author stated that as so many exaggerated reports had appeared lately in the newspapers as to the wonderful results that are expected from the great reflecting telescope proposed for the 1900 Paris Exhibition, he thought it might interest the Royal Dublin Society to know what might reasonably be expected from such an instrument.

According to some papers, the Moon is to be brought within 4 miles of the Earth, while in one account, purporting to be extracted from the 'Morning Post,' this telescope was to be capable of taking photographs of celestial objects on a scale 10,000 times larger than possible by any existing telescope.

After describing the general features of the proposed Paris telescope, showing how the light-collecting power and magnifying-power of telescopes can be estimated, why it is possible to use high powers with larger apertures, &c., the author showed that the light-collecting power of this Paris telescope will be only about 50 per cent. greater than that of the 40-inch refractor of the Yerkes Observatory, Chicago, or, more strictly speaking, 35 per cent., allowance being made for the light lost by the reflector. In magnifying-power, or power of bringing objects apparently closer, and possible scale of photographs, it will only exceed the Yerkes instrument by 20 per cent., while in both respects it falls far below the reflecting instrument of Lord Rosse erected more than half a century ago.

In answer to questions put by some of his audience the author gave a more detailed account of the construction of the proposed instrument, and stated that he had been consulted in the earlier stages of its conception, but that he had felt bound to report against the feasibility of some of the proposals of the promoters. He was not aware whether their proposals had been modified; but the original idea, as explained to him, was to have a magnifying combination applied at the breech piece, which would throw images of the nebulæ and other celestial objects on a semi-transparent screen 90 feet in diameter, behind which was to be built a theatre to accommodate 600 persons, so that that number of persons could simultaneously enjoy a telescopic view of such objects thrown on a screen of 90 feet diameter!!

On cloudy nights, or nights otherwise unsuitable (which, under the circumstances, would probably be rather frequent), it was proposed to substitute an electric lantern and project photographs of celestial objects on the screen; which would probably be quite as satisfactory to the spectators.

OBSERVATIONS OF THE LEONIDS AND BIELIDS AT GENEVA.—Monsieur R. Gautier, Director of the Geneva Observatory, kindly communicated to us the results of meteor observations made last November at the fortifications of St. Maurice, Switzerland. We regret that we have not been able to publish these before. Acting in co-operation with the Bureau of Fortifications, a plan of observation was organized, and Lieut.-Col. Dietler, the Chief of the Bureau, gave instructions to the non-commissioned officers to make observations at Savatan at an altitude of about 700 metres, and at Dailly, which is at an altitude of about 1250 metres. The night November 13–14 was partly cloudy, especially at Savatan, but the next night the weather was more favourable. The results are given in the following table:—

November 14.

Central European Time.	Number of Meteors.	
	Dailly.	Savatan.
h h		
6½– 7 ..	2	..
7 – 8 ..	6	..
8 – 8½..	2	..
8½– 9 ..	4	6
9 –10 ..	5	13
10 –11 ..	12	12
11 –12 ..	21	24

November 15.

Central European Time.	Number of Meteors.	
	Dailly.	Savatan.
h h		
12 –1 ..	22	23
1 –2 ..	16	15
2 –3 ..	33	28
3 –4 ..	28	60
4 –5 ..	51	87
5 –6 ..	60	86
6 –6½..	28	23
6½–7 ..	7	..

As to the Bielids, the reports were less satisfactory. Fog prevailed at Savatan from November 22 to 24, and at Dailly on the first of these nights. On the night November 23–24 the observers, who were stationed at the top of the mountain at an altitude of 1500 metres, noted ten shooting-stars from midnight to half-past one, and five from $2^h\ 20^m$ to $3^h\ 45^m$. It is difficult to say if these were Bielids. The observers did not consider these more brilliant than those seen on the nights November 13–15.

THE LOWELL OBSERVATORY. — In the February number of 'Popular Astronomy' there was an article, read by Mr. A. E. Douglas at Boston in 1898, August, on the planetary work done at Lowell during the last four years; the results, of which a bare summary is given below, may appear rather sensational, but their possibility is ascribed to the excellence of the observing conditions at Arizona, which are analyzed at some length in the paper:—

Mercury.—Schiaparelli's result that the planet rotates once during its revolution round the Sun was confirmed.

Venus.—Venus also was found to continuously present the same aspect to the Sun. The straw-colour is ascribed to the presence of an atmosphere.

Mars.—Several new canals and lakes were found, and the seasonal changes on the planet were observed. The South Polar Cap was seen to diminish as the equinox approached and a dark line formed round it, which suggested growing vegetation. The projections on the terminator are considered by W. H. Pickering to be clouds; if they appear only in the Martian night, as he suggests, this explains in some degree the high mean temperature of the planet.

Vesta.—The disk of this small planet was measured and the measures were found to indicate a polar compression of $\frac{1}{10}$, the major axis being almost in the direction of the orbit! Detail has been suspected which would indicate a direct rotation in less than 30 hours!

Jupiter.—Markings were seen on the *satellites* from which their rotation was deduced. Satellite I. rotates in $12^h\ 24^m$. For satellite II. no time is given, but the fact of rotation was established. Satellites III. and IV. have direct rotation and always present the same face to Jupiter.

A NEW METHOD OF DETERMINING FLEXURE.—It is obvious that some researches with a transit-circle would be received with more confidence if there were means of determining direction with respect to the surface of the Earth other than that of the nadir. A zenith collimator is used at some observatories, but if it were possible to set the telescope in a direction which was known to be 45° from the vertical, there would be at once a valuable means of determining flexure. MM. Perchot and Ebert, of the Paris Observatory, have made an attempt to arrange such a method ('Comptes Rendus,' 1899 March 6) by means of a float, in an annular basin, with a mirror attached approximately inclined 45° to the vertical. The *exact* inclination is determined by means of the telescope, the horizontal collimator, and a vertical collimator below the basin. When this angle is determined, the basin with the float and mirror is to be raised, so that the principal telescope inclined at an altitude of 45°, can be set on it and the image of the wires reflected at the mirror may be made to coincide with the wires themselves, just as in nadir observations. The method is ingenious; we shall be glad to hear of its successful application.

SIR ROBERT BALL'S LECTURE AT BLACKHEATH.—We do not usually refer to the Lowndean Professor's lectures, because this is intended to be a serious astronomical Magazine; but as Sir Robert Ball lectured on March 15 within, as he said, the shadow of the Royal Observatory, we felt it our duty to attend as Reporter. The facts leave no room for doubting that Sir Robert is a popular and superb lecturer. The large Concert Hall at Blackheath was crammed, and we believe that it could have been filled if it had been twice as large. The lecturer kept his audience of more than a thousand people in rapt attention, broken only by bursts of laughter, for almost two hours, by means of some ingenious mechanical slides, his happy style, and his genial and occasionally dramatic oratory. It is unnecessary to attempt any description of the matter of the lecture, but these two *voces populi*, which were actually overheard, are illustrative of general opinion:—

Man going in, meeting a professional astronomer. Well! I didn't expect to see *you* here.

Young lady coming out, to her companion. Fancy mother thinking it would be *too scientific* for us!

SOLAR ACTIVITY.—During the first two months of the current year the Sun's disc was almost absolutely free from spots, so that it might have been thought that the minimum phase of the period had arrived. This condition of quiescence has, however, been disturbed during the past month, for a spot of considerable size in latitude 7° S. came round the visible edge of the disc on March 15. The spot was seen on the Greenwich photographs on March 15 and 19, and when it reached the central meridian on March 22 there was a slight disturbance of the magnetometers. We have received an interesting account of this spot, with some excellent diagrams, from Mr. W. Anderson, of Madeira, who observed some remarkable cyclonic changes in the group. We are obliged to hold over his article until next number.

WINTRY WEATHER IN MARCH.—The expectation of a mild spring, held out by the remarkably warm weather experienced at the beginning of February, was somewhat rudely shaken by the cold period which occurred at the end of that month, and still more so by the severer period which has recently occurred, extending from the 18th to the 25th of March. During this period of 8 days the daily minimum temperature averaged 25°·7, the lowest value being 20°·3 on the 21st, and the mean temperature 33°·0. Low temperatures are not infrequent in March, although scarcely to be looked for at so late a period. In 1850 on the 26th and 28th of March, however, temperatures of 20° and 21° were recorded, and at earlier dates in the month there are several instances of lower temperature: for example, 13°·1 on March 4 in 1890, and also on March 14 in 1845; 16°·9 on March 11 in 1847; 18°·1 on March 6 in 1845; and 18°·7 on March 4 in 1889. Long extended periods of cold during the latter part of the month of March were recorded in 1865 from the 18th to the 31st, and in 1853 from the 17th to the 29th, the mean minimum temperatures for these periods being 28°·1 and 26°·5 respectively.

HERR LEO BRENNER has kindly sent us a copy of the first number of his new astronomical publication, 'Astronomische Rundschau,' of which announcement has already been made. If the standard of this first issue is maintained, it will deserve the support of all followers of the science in Austria. The number contains a long account, with illustrations, of Herr Brenner's observations of Saturn; a description of a new observatory at Altenburg, with pictures; a history of Eros; a biography of Schiaparelli; and Notes. There are a considerable number of advertisements, which help to support the journal; if it were not for these we should wonder that so much matter could be given for the subscription.

FROM the Report of the Cape Observatory in the February number of the 'Monthly Notices' we learn that Mr. Finlay, who has been Chief Assistant there since 1873 July, has resigned his position on account of ill-health.

WE make it a rule to record any astronomical allusions in contemporary literature. In *Punch* for March 29 there is a picture showing a reveller returning homeward. As is the custom with revellers in comic papers, he sees *two* moons, and, his thoughts wandering to recent newspaper paragraphs about the Earth having one Moon, Jupiter five, and Saturn nine, he is quite in doubt as to which planet he is on.

Mr. G. W. Walker, of Trinity College, has been elected to the Isaac Newton Studentship in Astronomy and Physical Optics in the University of Cambridge.

We are informed that Prof. T. J. J. See has severed his connection with the staff of the Lowell Observatory (and has been appointed Professor of Mathematics at the Naval Academy, Annapolis). His permanent address is now: Nautical Almanac Office, Naval Observatory, Washington.

Errata.—Mr. Lynn asks us to correct the word "Prose" in the 5th line of his letter on p. 127 in our last number; it should be "Phrase." He also calls attention to the circumstance that, in speaking of the comet discovered by Mr. Forster, at the bottom of p. 319 of Vol. xx. (August, 1897), the year of its appearance should have been mentioned; it was 1819.

From an Oxford Note-Book.

From far California comes a remonstrance dictated by one of the notes in the February number, which apparently gives a false impression:—

> In your February notes, pp. 107, 108, you give the impression (I think) that with modern photographic appliances the astronomer has only need to "put in the plates." At least, I understand you as concurring with Sir R. S. Ball to this extent. Now, perhaps your equatorial machinery is so perfect on the other side of the water, that driving-clocks give as good results as chronometers: but, as I remember my friend Barnard, when photographing at the Lick Observatory, his tireless eye was glued on the cross-hairs of the finder during the lengthy exposure with a far greater persistence than when observing, for he did not dare to rest a second for fear of movement. Wouldn't this act gain the sympathy of the "young ladies who do not know anything about astronomy" (and they are found outside of England)?

If this impression was given (and of course the reader is a better judge than the writer in such matters), both Sir R. S. Ball and myself would, I am sure, wish it speedily corrected. Neither at Cambridge nor Oxford is there as yet a driving-clock which will allow the observer to go to sleep with satisfactory results; and both in the lecture and in the note on it we were, I fear, deviating from the narrow line of serious and exact truth. At the same time a good deal of astronomical photography is done without supervision by the observer, and more ought to be. The telescope working by itself while the astronomer sleeps represents certainly an ideal, but an attainable ideal, to which a considerable approximation has already been made.

THE troublesome discordance called the R—D discordance has been attributed to a variety of causes, none of them as yet affording a completely satisfactory explanation. Personally I am convinced that irregular refraction near the instrument is *not* the cause; and that the true explanation is to be found in some sort of instrumental strain which develops gradually with the time during the minute or two after the setting of the instrument in any definite position. It may easily be, as suggested in a recent paper * by Mr. J. R. Eastman, that the seat of this strain is the mounting of the object-glass. He says:—

> If we examine the history of the telescope as well as the structure of the modern instrument, it will be seen readily that it was designed for use on objects above the horizon. If the objective in its cell be carefully fitted into the collar on the end of the telescope tube, in the usual way, it is probable that, for work on all objects *directly*, the objective would, simply by the action of gravity, maintain its position with relation to the telescope tube. But when the telescope is pointed below the horizon for observing the nadir or a reflected star, all the conditions are immediately changed.

WHETHER the usual method of mounting an object-glass is responsible for the R—D discordance or not, it is tolerably certain that many transit-circle object-glasses (and especially small ones) are not satisfactorily mounted; on several occasions within my own experience puzzling errors have been traced to this source. Small object-glasses are often held in metal cells screwed up, fairly tight, but of course not so tight as to risk breaking the glass, and with nothing of a springy nature to take up any looseness following on subsequent changes of temperature. They are thus apt to come loose, in a manner which has no appreciable effect on equatorial observations, but may introduce sensible errors into meridian work. There ought always to be a strong spring of some kind, either metallic or otherwise. As yet I have found nothing to beat cotton-wool packed tightly between the glass and its cell; though this is not a satisfactory permanent arrangement.

We have tried various devices with the 4-inch object-glass of the Barclay Transit Circle at the University Observatory, without success, until 3 months ago a cotton-wool packing was adopted; since then all the readings for collimation have been within a second of arc.

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* Astronomical Journal, No. 454.

Since Prof. Newcomb's retirement from office we have had a number of short papers in the *Astron. Journal* and *Astr. Nachrichten*, representing probably various notes which have been lying on his desk awaiting some period of leisure for completion. I imagine most busy men have a collection of these on their tables, and we should be the wiser and richer if routine could be stopped for a year or two so as to allow of their publication; for the "next week," when it is hoped to finish off this or that thing, so seldom comes. Those who visit the busy man in his den may hear of these floating ideas, or may see some half-constructed instrument lying in his workshop, which may set them thinking, but the world at large does not get the benefit of them until the busy man retires from office, and often not even then; for men like Prof. Newcomb, who properly use this compulsory leisure, are after all something of an exception.

The Report of the U.S. Naval Observatory for the year ending 1898, June 30, seems to show that the war with Spain found out the deficiencies in the Chronometer department.

> Many chronometers "were issued to vessels after a trial of only two or three weeks at the Observatory. The supply of new chronometers in this country was soon exhausted and 25 were ordered by cable from London. Of these latter 10 were issued direct from the New York Navy Yard, the other 15 coming to the Observatory. These 15 proved fairly satisfactory, although nearly all that remained here many weeks showed a considerable acceleration of rate, as most new chronometers do."

Some years ago we should have had similar trouble in England had a naval war broken out unexpectedly; but fortunately, thanks to the energy of the present Hydrographer and the present Astronomer Royal, we are now fairly well equipped in the matter of chronometers, and the work of the Royal Observatory has grown in consequence, as those who visit the Observatory may learn from Mr. Lewis, if they have the curiosity to ask. By-the-way, is the 26-inch Washington refractor being put to the best uses in measuring Titan and Rhea, or in determining diameters of Venus and Mercury? Of course this is good work and better done by a large telescope than a small one; but is there not other work for large telescopes more urgently needed?

Congratulations to Photography, to the Harvard Observatory generally, and to Prof. W. H. Pickering in particular, on the new satellite of Saturn. What is it to be called, please? I hope it will be a good long name, because there is an old man at the sea-side who shows me the satellites of Saturn, for one penny, through his (very respectable) telescope, who gives all their names correctly until he comes to Iapetus, which he always calls Japonica in a delightful way; and I think he ought to have a chance to transmute some other long name. I may mention here, that an astronomer has been deservedly canonized. An examinee told me recently that the satellites of Mars were discovered by "St. Asaph Hall" in 1877.

THERE has been an unusually luxuriant crop of paradoxical literature lately; several pamphlets and newspaper articles with ambitious claims having come to hand. "A member of the Chicago Astronomical Society" claims to have measured the Earth; but his method seems to be the old one of just thinking about it and settling in his own mind how big it is. The size depends on a certain angle θ, which has a value somewhere near 140°, and "after having worked with several values of the angle, a labor which occupied what otherwise would have been the spare time of many weeks, the writer has concluded that $\theta = 144^\circ{\cdot}0 = 4\pi/5$; no more no less." I have found it an interesting exercise to try and fathom the meaning of the following sentence in his essay:—

> He (the M.C.A.S.) considers that 2·63 may fairly be taken as the surface density which is normal to the forces of gravity and rotation, they being the only ones that we have a right to regard as operating below the comparatively thin shell in which alone oxygen and water can affect the chemical composition of the strata and alter the specific gravity.

ANOTHER set of pamphlets detail an attempt to supersede Bode's Law. The titlepages are headed "A la Conquête du Ciel! Contributions astronomiques de F. C. de Nascius en quinze livres." It is the second of these 15 books which is devoted to the law of planetary distances; and the idea seems to be, that if you write down the series of powers of Q (where $Q^4 = 2$) you can always find two or more of them which when added together and divided by something, or divided into something, will give the distance of a planet from the Sun. Thus $\frac{1}{2}\,(Q^{19} + {}^{20}) = 29{\cdot}45$, while the distance of Neptune from the Sun is, by a marvellous coincidence, 30·05, The author claims that this law is not empirical like Bode's, but is a natural consequence of Kepler's third law; that it is not much less simple than Bode's law and is destined to become much more popular. It shows, if I understand him rightly, that Vulcan and an ultra-Neptunian planet cannot legally exist, but that there are exactly 980 minor planets; and if so, the author is certainly justified in winding up one of his pamphlets with the following song of jubilation:—

> Oui, tout homme verra, mis en pleine évidence
> Le Plan de l'Univers ainsi que je le vois:
> Car, pour que cela fût, la Haute-Providence
> Le voulut à merci d'une règle de trois!

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1899 April 14.

Prof. G. H. Darwin, LL.D., F.R.S., *President*, in the Chair.

Secretaries: H. F. Newall, M.A., and F. W. Dyson, M.A.

The Minutes of the previous Meeting were read and confirmed.

Mr. Dyson. 80 presents have been received since the last Meeting. Most of these are periodicals, but several of them are worthy of particular notice. There is an interesting old book, 'Astronomici Veteres,' presented by Mr. W. Arthur, published in 1449 by Aldus, a very beautifully printed old astronomical book. There are several large works of the nature of catalogues, such as the 'Poulkova Catalogue of Fundamental Right Ascensions'; some observations by Hermann Struve of the satellites of Saturn; and catalogues of stars from Vienna and from the Dunsink Observatory.

The thanks of the Society were accorded to the donors.

The President. It may be within the knowledge of those present that it is proposed that the University of Cambridge should celebrate in the coming June the 50th year of the tenure by Sir George Stokes of the Chair of Lucasian Professor. The University of Cambridge has invited this Society to send a delegate on that occasion, and the Council has done me the honour of nominating me as that delegate. An address of congratulation to Sir George Stokes is now in course of preparation, and will be presented by the Society on the 1st or 2nd of June next, on which of these days may be appointed for the reception of such address.

Mr. Dyson then read a paper by *Mr. J. L. Scott*, of Shanghai, on the measures of southern double stars 1897–98. The paper was hardly one for reading, as it was simply a record of observations of double stars made in the years 1897–98, at Shanghai, by Mr. Scott with his 5-inch object-glass by Casella, mounted by Cooke & Sons. He used a double parallel wire-micrometer by the same makers. The large number of observations made in the course of two years' work comprised southern stars in nearly all cases. By reason of the size of the telescope the distances of the stars apart were seldom less than 2 seconds, but the observations seemed to be very accordant. There were more double-star observers in the northern hemisphere than in the southern, and this paper was a valuable contribution to double-star observations. Mr. Scott had sent his object-glass home to Messrs. Cooke for repolishing, but he proposed to continue his observation of double stars in future years.

Mr. Lewis. I should like to remark about this paper that it, perhaps, starts a new era in double-star work amongst Englishmen. We have almost entirely given it up. We are doing some at Greenwich with a large instrument, but Mr. Maw is probably the only other double-star observer we have. There is a general idea abroad that because most of the large instruments of the world are devoted to double-star work, there is no room for a small instrument. That is a fallacy. There is more work for a small instrument now than there ever was. Take the Struve Catalogue: in that there are 2800 stars, and the number of stars observed at Greenwich out of that catalogue would not be more than 200. It is the same at the Lick and at Yerkes, so that there is a very large number left for small instruments, and it is very satisfactory to find Mr. Scott working on that line. I understand that these are also southern doubles. There are not enough observers in the southern hemisphere to-day, and I think that Mr. Scott is to be congratulated on sending us this paper.

The President. It is interesting to find that at Shanghai they have something else to do than squabble with the French about concessions. I am glad to find that there are people in that part of the world who are willing to devote their leisure to these researches. What Mr. Lewis has said about small instruments is very interesting.

Dr. Isaac Roberts (reading a paper on the "Leonid Meteor Stream"). On the night of the 13th, and morning of the 14th, November last, I watched the sky with the object of photographing the meteor-stream, but the sky was clouded over, excepting for an occasional momentary break, till 2 o'clock on the morning of the 14th, when clear sky prevailed from that time until daylight. During that interval two photographs were obtained with the 20-inch reflector, one with an exposure of 90 minutes, and the other with two hours' exposure; but there was no indication of the meteor-stream on either of them. Two photographic plates were also exposed simultaneously with them in the 5-inch lens camera, and although two meteor-trails were observed during the exposure of the plates, they were outside the range of the photo-field of the instruments.

Two new nebulæ were discovered on the plates in close proximity to the meteor-radiant. The following are their co-ordinates as deduced from the star D.M. $+22^\circ{\cdot}2164$, R.A. $9^h\ 54^m\ 44^s{\cdot}4$, Dec. $+22^\circ\ 39'{\cdot}7$. Epoch 1855·0.—The nebula (1) is like a star of about 13th mag. surrounded by a halo of faint nebulosity in R.A. $9^h\ 55^m\ 54^s{\cdot}8$, Dec. $+22^\circ\ 59'{\cdot}4$. Neb. (2) is of about the 16th mag., elongated in *preceding* and *following* direction, and has the same R.A. as (1), but is $43''$ south of it.

Three attempts have also been made to photograph the Leonid Meteor-stream in space, using the ephemeris provided by Dr. G. Johnstone Stoney and Dr. Downing, Mr. Wright, of the Nautical Almanac Office, being the computer. The attempts were made on the nights of 1899, February 16, exposure 90 minutes; on March 5th, exposure two hours; and on March 10th, exposure four hours. No indication of the meteor-stream was to be seen on any of the photographs, and therefore I infer that they are fainter in their photographic effects than stars of the 17th or 18th magnitude.

I would suggest to those who will engage in the work of photographing the meteor-trails, during the apparition in November next, to find the equation of their instruments and of their plates during the months between now and November, when the Moon is 11 days old, exposing the plates to a part of the sky of declination about 23° and about 135° from the Moon, during respective intervals of 5, 10, 15, 20, and 30 minutes. In this way they will gain experience and be able to determine, before the critical time arrives, what exposures may safely be given to the plates without spoiling them by over-exposure during the moonlight which will prevail when the meteors are falling.

Dr. Stoney. I think both the Society and astronomy are very deeply indebted to Dr. Roberts for the laborious efforts he has carried out for photographing the Leonids while still in the open sky. There have been only two opportunities when it was possible to predict the return of the Leonids to our Earth. This is the second of those occasions. On the first occasion, 33 years ago, observations such as Dr. Roberts has carried out were impossible, because it was not until after the swarm had nearly passed the Earth that the orbit in which they move was determined. The present, therefore, is the first opportunity on which observations of the kind Dr. Roberts has made were possible. Of course, all astronomers anticipated that the light which would come from the Leonid stream is excessively faint; and although it has not been possible to photograph them, it is a distinct step in advance to know not merely from general considerations, but from actual observation, that they are so faint that it has proved to be impossible to photograph them in the climate of England. Now, when the extreme front of the stream or the *Ortho-Leonids*—

meaning by Ortho-Leonids those which pursue nearly the same orbit and which form the dense part of the ring of meteors—reached the Earth last November it is most unfortunate, and I take to myself the full share of regret for the oversight, that no person thought of pointing a camera in the direction which would take the stream end on as it approached the Earth on that occasion. No opportunity of doing this in the absence of moonlight will occur now until the year 1901; but this year and next year, although again moonlight will interfere at the critical moment, it may be possible to get the direction of the tangent of the stream when moonlight is absent (some days before and some days after that date); and if that tangent passes sufficiently close to the Earth the stream will be seen on those occasions nearly end on, and this will offer opportunities more favourable than any of those yet employed for taking a photograph of the stream in the open sky. Dr. Roberts has most kindly consented to make the trial next November if the requisite ephemeris is prepared, and I hope the necessary ephemeris will be provided. A very remarkable case occurred lately in which a medal was awarded by the Royal Society to Prof. Oliver Lodge for a valuable and difficult course of investigation which led to an absolutely negative result, and I think the same kind of recognition which was given to Prof. Lodge is now due to Dr. Roberts for the very laborious efforts he has made to endeavour to determine whether the light from the Leonids in the open sky is sufficient or not to impress a photographic plate.

Mr. Dyson. Might I ask Dr. Roberts whether he can give an idea of the possible brightness as compared with Saturn's ring? Can he give us the limit of the brightness of this meteor-stream? Can the brightness be as much as one-ten-thousandth or one fifty-thousandth part of that of the light of Saturn's ring? and in what time could he photograph the ring with his reflector?

Dr. Roberts. I have not considered that point.

Mr. Dyson. Let us suppose it would take one second, and the shower just cannot be photographed, the ratio of brightness would be 1 to 3600.

Dr. Roberts. I have not aimed at trying what is the shortest time in which the ring can be shown without the light of the planet itself. There is a halo round the bright object in the sky owing to atmospheric effects, and that halo affects the film to a large extent.

Dr. Rambaut. What is the field covered by the lens used in conjunction with the reflector? Dr. Stoney referred to the fact that nobody had pointed their photographic cameras to a point 7° from the radiant-point. If Dr. Roberts's plates included that point, then the swarm, if it existed there and were bright enough to be photographed, ought to have left a trace on Dr. Roberts's photograph.

Dr. Stoney. The photographs should have been taken at the time when the shower came—that is, on the morning of November 15. I think it is perhaps a correct statement that a meteor-stream would affect the photographic plate as if the individual meteors were perfectly stationary, because they follow one another at such short intervals and are so close to one another laterally that the impressions made by the individual meteors are large enough to overlap.

The President. I was much struck by Dr. Stoney's remarks upon the importance of failures in science. There is a great deal of the most important scientific work which consists in work that is apparently not successful. When we hear of the elaborate care which Dr. Roberts has bestowed upon his preparations, we realize that his reputation as one of the pioneers in the great advance of photography in astronomical science has not been earned by sitting still in an arm-chair. Our thanks are due to Dr. Roberts for his communication.

Mr. Newall then gave an account of some new spectroscopic determinations of velocity in the line of sight. He said that, in the hope of being able to complete a paper for formal presentation to the Society, he had had some lantern-slides prepared to give some idea of the photographs of stellar spectra from which the determinations are derived at Cambridge, and it might be of interest to the Fellows to see some of them. (Photographs shown on screen.) He pointed out that each line measured in the stellar spectrum served to give a determination of the velocity, and explained how discrepancies arose from the fact that a line which, in the small scale of dispersion used in stellar spectra, looked like a definite line directly comparable with the corresponding iron-spark line, was in reality a group of lines in which the true position of the iron line was masked by the presence of neighbouring lines. He had formerly attempted to apply a correction for this, but in order to make out something more about the cause of discrepancies of determinations deduced from the separate lines in a given spectrum, he had been devoting considerable time to measurements and reductions of these spectra in a different way from that originally adopted. His present method was to deal with a great number of lines in each of a few typical spectra, with the view of being able to analyze the separate determinations into groups corresponding with different chemical substances. For instance, the velocity derived from all the iron lines is compared with that derived from the calcium lines. Thus, in the spectrum of α Cygni he was able to deal with 40 or 50 lines, and in that of α Persei with 150 lines. The wave-length of each line was determined with respect to standard lines in the comparison iron-spark spectrum and compared with the wave-length of lines of known origin. The difference in wave-length was then expressed in terms of velocity.

Mr. R. A. Gregory. Do I understand Mr. Newall to suggest that if he uses the calcium lines and the iron lines as reference lines, there is some indication of a difference in the results. One does not understand why there should be.

Mr. Newall. There is a known effect produced by pressure upon the wave-length of lines in the spectrum. The work of Humphreys and Mohler seems to have established the fact that an increase of pressure in the vapour in which the line originates alters the apparent wave-length. If, then, a difference in the wave-length can be produced, not only by the relative velocity of the star and the Earth, but also by the difference of the pressure of the vapour which has given rise to the lines in the star, it is possible that results derived from iron lines may differ from those derived from calcium lines in the same star. We may, in fact, possibly be able to express these discrepant velocities in terms of a common velocity, plus or minus a variable-pressure effect.

The President. Such investigations as that of Mr. Newall are being carried out with the object of determining the velocity of various stars, and no doubt similar investigations are being carried out by a considerable number of astronomers, but yet the total amount of work that has as yet been done is as nothing at all compared with what may be done in this class of research. If all astronomers were to devote their energies to it for a century or two, we still should have barely a plan of all the velocities of the star world.

The Astronomer Royal, presenting a paper on the Results of Micrometer Measures of Double Stars made with the 28-inch Refractor at the Royal Observatory in the years 1896–98, said some of the results might perhaps have been published earlier. In making the working catalogue a careful selection was made of those stars which would be the most suitable for the powers of the instrument, which is of considerable aperture, and therefore the catalogue was formed of well-known binaries, suspected binaries, and difficult stars not in reach of a small instrument. Care was taken not to observe double stars promiscuously. Altogether 414 stars were observed. There were 96 under ½ second apart, about three-quarters of them were under 2 seconds, and there are a number of others over 2 and under 5 seconds, which were observed as being interesting objects for other reasons. In some cases the third star near to a close pair was observed. Besides this there were systems in which the principal star was bright and the companion faint, being therefore difficult objects, such as Sirius and Procyon.

One star in particular, γ^2 Andromedæ, during these observations changed 170 degrees in position-angle. It is a very close double, and the star passed from one side to the other in a very elongated ellipse. The observations were made by Mr. Dyson, Mr. Lewis, Mr. Bryant, and Mr. Bowyer. Mr. Lewis, Mr. Bryant, and Mr. Bowyer took the lion's share of the observations, and Mr. Lewis is mainly responsible for the selection of stars; and as he takes particular interest in these double stars, I think he might be able to explain some of the points of interest in the observations.

Mr. Lewis. I do not know that there is very much to add to what the Astronomer Royal has said. Classifying the stars in order of separation only gives a very rough idea as to the difficulty of the work. There are several stars under ½ second that one can observe on almost any night, whereas there are stars 5 seconds apart you would not attempt under the same conditions. So much depends upon the relative magnitudes. For instance, in the case of Sirius, the separation is about 5 seconds, but you cannot observe it very frequently, while such stars as Burnham 525, three- or four-tenths of a second apart, you can generally see and measure. I do not know whether Mr. Maw has noticed the pair Σ 1957 separated about one second of arc. They seem involved in nebula. On one very bright night I could not get any definition of these two stars, while those in the immediate neighbourhood were well defined. Struve does not mention it, but his observations are rough compared to what they ought to be. Then there is κ Pegasi, a star we have observed very much at Greenwich in a part of its orbit in which there are no other observations published. Another interesting star is β 883, whose period is 5½ years. There was some doubt when it was given out as 5½ years. Our observations really do show that the star has reached its elongation and is going back, so whatever the period is it must be extremely short. It might be as well to mention that long series of measures of 70 Ophiuchi have been made, at the request of Dr. See, with a view to establish the supposed irregularity in the orbit itself.

The President. Can the Astronomer Royal or Mr. Lewis tell us approximately how many known binary stars there are in the north and south hemispheres? I mean double stars connected physically with one another. Is it in the hundreds?

The Astronomer Royal. I am not prepared to answer that offhand; it is a question one wants to go into. There are a certain number of well-known binaries, but there is a very much larger number which are debatable. There are others which have a common proper motion, but do not show orbital motion. I should think that for one known binary there may be ten which are doubtful.

Prof. Turner read a paper on the Errors of Star-Photographs due to Optical Distortion of the Object-Glass with which the photograph is taken. The present paper deals with the possible displacement of star-images due to various causes, chiefly optical distortion of the lens on the star-photographs. The interest in measurement of photographs of stars and other heavenly bodies is certainly not on the wane, and it becomes more and more necessary to enquire what is the accuracy with which stars are delineated by the photographic method. In 1870, when the Astrographic Conference met, there was not much information available as to the degree of accuracy of photographic positions. When it came to be a question with what instrument the Astrographic Chart was to be made, the photographic doublet was put on one side as likely to have rather complicated optical distortion; and although it had a very much smaller field, the simple refractor was preferred and the Astrographic Chart was undertaken with refractors. At the Conference, Professor Pickering, of Harvard, urged the claims of the photographic doublet, which gave a very much larger field, but, in view of distortion, the refractor was chosen. Little has been done in the way of examination of the optical distortion of different instruments. Soon after the Conference met, Dr. Scheiner, of Potsdam, published a few measures, which showed that the refractor did not give a distortion over more than one degree square. That did not, however, take us very far; but it soon became evident that over two degrees square we could depend upon the distortion being very small. The first part of this present paper gives complete information as to the freedom from optical distortion of the ordinary astrographic instrument which has been in use at several observatories for some time. Over 500 plates have now been measured at Oxford and the results compared with meridian observation. Meridian observations have errors of their own, but they are independent of the position of the star on the photographic plate. A number of plates were accordingly divided up into 16 parts, and the mean taken of the residuals in R.A. and declination. (Blackboard.) They are seen to be quite irregular or accidental, and the largest is about $0''\cdot15$, or half a unit of measurement. All the measures were made by scale reading to $3''\cdot0$ and *by estimation* to $0''\cdot3$. We may take it that the plates are quite free from optical distortion of any kind. The next question is over how large a field can we get photographs similarly free from optical distortion by using a doublet. Prof. Pickering has kindly sent me some plates taken with the Bruce doublet at Arequipa. They are too large (16 × 13 inches) to go directly into our measuring-apparatus, which is intended for 6 × 6 inch plates; so copies were taken of portions of the large plate, in each case corresponding very closely with a plate taken at Oxford and already measured. The copies were measured and compared with the Oxford measures, and the differences in x and y were expressed as far as possible by linear expressions, $ax+by+c$ and $dx+ey+f$, representing differences of scale-value, orientation, centring, and refraction. They were also corrected for the inclination of the planes of the two plates, according to formulæ which have been given in previous papers. The residual differences were small but sensible, amounting to $1''$ or $2''$ in different parts of the plates. They were, in the first instance, considered to represent the optical distortion of the photographic doublet, but no centre of distortion could be assigned so as to satisfy the residuals, and it was at last shown that they could not be due to *any* optical distortion radiating from a centre, or any combination of such radial distortions. The residuals could be expressed very nearly by two expressions of the second order in x and y, such as $Ax^2+Bxy+Cy^2$ and $Dx^2+Exy+Fy^2$; but it is shown in the paper that if they were due to distortion there would be relations between the coefficients A B C D E F, which are not even approximately fulfilled by the present residuals. Thus it is shown in the paper that for optical distortion we must have $B=2D$, $E=2C$, and the observations could not in any way be twisted so as to get these relations. I was convinced that the residual differences must be traced to some other cause, and this was ultimately found in the considerable curvature of the glass on which the Arequipa positive was made. I believe that in making the copy at Oxford (possibly also at Arequipa) only partial contact between the films was secured; and unless the light fell accurately normal to the common surface, we should thus have errors introduced in the copy at places where the films were separated. A separation of 0·2 mm., and a beam of light inclined at 6° to the normal, would give a displacement of 0·02 mm. or $1''\cdot2$; and measurements of the curvature show that a separation of this amount at the time of copying is quite possible. Further, a simple criterion for detecting errors of this kind is that the errors in x should be directly proportional to those in y, and this criterion was found to be fulfilled by the residuals. Hence I believe the optical distortion of the Arequipa plate over the portion already examined, which is about $4^\circ \times 4^\circ$, to be insensible. But further examination is necessary, with precautions against these errors due to curvature of the glass.

The Astronomer Royal. I am very glad to find that Prof. Turner has taken up this important question of optical distortion as it affects stellar photography, and I am sure all the Fellows present have listened with very great interest to his explanation of his researches. I am entirely in agreement with him as regards the results he has obtained, because two points he has brought out particularly about the curvature of the plates agree exactly with the experience we have had at Greenwich. The point is that the curvature of the plate is a very serious difficulty, and the larger the plate you deal with, and the larger the field you deal with, the more you are exposed to this difficulty of curvature. If you look at the residuals which Prof. Turner has obtained you find that whereas with plates of moderate size he finds a maximum residual of $0''\cdot5$, with a larger plate he gets $1''\cdot2$, which is a very large increase. So the question comes to this—what are we prepared to sacrifice in order to get a larger field? and that is an important point. I am inclined to think that the field adopted for the Astrographic Chart—that is, two degrees square—is the largest field we can use to get the greatest accuracy obtainable in photographs, and also to get their highest defining-power over that field; but if we want for other purposes to get a pictorial representation of the sky to map out the heavens in a rapid way we must be prepared to sacrifice something, and I am one of those who think that it might be worth while to sacrifice a good deal in order to photograph the whole heavens more frequently, and thus get more information. When we deal with a field of four degrees square instead of with two degrees, we have to face some difficulties. In the first place, we must sacrifice definition, because I do not believe the optician has yet succeeded in getting as perfect definition for a field of four degrees as can be obtained for a field of two degrees. In each case we have to compromise. The plate has to be put into a position which will not give the very best definition in the centre of the field. We have to equalize things and put the plate so as to get the best general results. The larger errors I do not think are due so much to photographic or optical distortion as to inequalities and distortion of the plate. There is another thing I will suggest for Prof. Turner's consideration. When we have made these measures over a field of four degrees, in order to reduce them to the sphere we have to apply corrections which can no longer be considered linear. We have to take into account small terms, and for the purpose of measurement I am not prepared to admit that plates of more than two degrees square can be practically utilized; but I should be very pleased if Prof. Turner can convince me that larger plates can be used with advantage.

Capt. Hills. This is a subject on which I have tried some experiments myself, but they are far cruder and rougher ones than those of Prof. Turner. These were made in connection with some experiments on the determination of longitude from photographs of Moon and stars, and the purpose was to investigate the distortion over a still much larger field than that Prof. Turner has used. The method I adopted was to allow the stars to trail across the plate. I used two lenses, one being the old-fashioned doublet, the other a Dallmeyer rectilinear. I was only considering distortion greater than 1″, and I found that up to five degrees from the centre I got no distortion with either lens. Above that the distortion of the doublet increased rather quickly and reached the maximum at ten degrees from the centre, whereas the Dallmeyer lens gave a small distortion. The result came out that the Dallmeyer lens was better when considering a field up to seven degrees square. The mean result I came to was that there was distortion up to five or six degrees from the centre, but in no case did it amount to a second of arc in either lens. This method of measuring distortion does not require any clock-driving.

Prof. Turner. As regards the inclination of the planes of two plates with different centres, corrections were applied to the Arequipa plate. The curvature introduced errors in the present case because of the method of copying, but I think you may have a plate curved if you copy it in the right way. I do not think the curvature need interfere with the plates giving good results.

The following papers were announced and partly read:—

Isaac Roberts. "Photographs of the Radiant of the Leonid Meteors, and Attempts to Photograph the Meteor-Stream."

J. L. Scott. "Measures of Southern Double Stars."

John Tebbutt. "Further Observations of Comet Coddington (*c* 1898)."

H. H. Turner. "On the Errors of Star-Photographs due to Optical Distortion of the Object-glass with which the Photograph is taken."

Royal Observatory, Greenwich. "Micrometer Measures of Double Stars made with the 28-inch Refractor in the Years 1896–98."

Royal Observatory, Greenwich. "Observations of Planet Eros from Photographs taken with the 30-inch Reflector of the Thompson Equatorial."

Radcliffe Observatory, Oxford. "Observations of Planet (433) Eros and of Comet Tuttle."

The following gentlemen were elected Fellows of the Society:—

E. W. Barnes, Trinity College, Cambridge.
Samuel Chatwood, Broad Oak Park, Worsley, near Manchester.
Rev. W. B. K. Francis, H.M.S. 'Boscawen,' Portland.
Capt. W. G. Lingham, 1 Caldervale Road, Clapham, S.W.

The following Candidate was proposed for election as a Fellow of the Society:—

Frederick E. Peach, 161 Stanstead Road, Forest Hill, S.E. (proposed by J. E. Evans).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The fifth ordinary Meeting for the current Session of the British Astronomical Association was held at Sion College on Wednesday, March 29, *Mr. W. H. Maw*, President, in the Chair.

Before beginning the ordinary business of the Meeting, the President alluded to the great loss which the Association had sustained in the death of Miss Elizabeth Brown, the Director of the Solar Section. Miss Brown, he said, was an original member of the Association, and took a most active part in its foundation, and she had since been one of its best friends. As Director of the Solar Section she had done most admirable work, and, apart from that, she had aided the Association in many ways.

A resolution expressing sympathy with Miss Brown's sister was passed by the Meeting.

The President then mentioned that the Council had received information that Miss Brown had left the Association a legacy of £1000 and one of her observatories with its contents.

Mr. Evershed read a paper on "Spectroscopic Research at Future Eclipses." He said that among the questions which present themselves for solution at future eclipses perhaps the most important was that relating to the distribution of the gases in the flash-spectrum layer. Although the entire thickness of the stratum subtended an angle of but 1″ or 2″, and consequently appeared at the moment of totality as an extremely fine thread of light, yet it would have to be examined in much greater detail than heretofore if an advance was to be made in knowledge of the relation which its bright-line spectrum bears to the dark-line Fraunhofer spectrum. The lower depths in which the photospheric clouds were suspended must be separated from the higher levels, to determine the order of succession of the various constituent gases in passing outward from the photosphere through the 800 miles or so of incandescent gases. Thus it would be of the greatest interest to learn whether in the lowest depths, where the pressure and temperature are greatest, the emission spectrum becomes more nearly the counterpart of the Fraunhofer spectrum, or whether, on the other hand, the dissociating effects of higher temperatures in those regions give rise to a simpler spectrum differing materially from the dark-line spectrum, as well as from the emission spectrum of the higher regions. He thought it would be possible at future eclipses to get fresh evidence bearing on this question, which should show, at any rate, which way the tendency lay with regard to the lowest strata. If observing stations were selected, not on the central line of the eclipse, but only a few miles within the north or south limits of totality, it was probable that some important results would be obtained. A calculation of the conditions which would obtain at the eclipse of May 28, 1900, at a station near the limits of the shadow zone, indicated that although totality itself would last only some twenty or thirty seconds, the duration of the flash-spectrum would be prolonged for much more than that period, and the covering up and uncovering by the Moon would be a very much more deliberate process. Another important point which future eclipse work would determine was the composition of the flash-spectrum with reference to the sun-spot cycle. Judging by the changes which seemed to take place in the coronal spectrum, it was to be expected that the flash would be richest in lines at the time of maximum sun-spots. The experience gained at the recent eclipse with regard to photographic plates was in one respect of great value to the spectroscopist. The advantages attending the use of triple-coated plates was clearly demonstrated by the successful photographs obtained by Mrs. Maunder, and in particular by the coronal photograph taken nearly two minutes after the Sun had reappeared.

In reply to *Mr. Maunder*, Mr. Evershed said he had made no selection of a station yet. He would go either to the north edge or the south edge of the shadow as might be most convenient at the time.

Mr. Crommelin said that if several observers would be self-sacrificing enough to go near the limits of the shadow on the occasion of the forthcoming eclipse, there were other observations besides spectroscopic ones which could be carried on with greater advantage there than on the central line. For instance, if they timed the beginning and end of totality at a station near the north limit, and also at a station near the south limit, it gave them the error of the Moon's place with very great accuracy both in right ascension and declination.

Mr. Downing remarked that the value of the Moon's diameter to be used in solar eclipse computations was still an uncertain quantity. The experience of the last few eclipses showed that the value used was somewhat too large. It was therefore very important that observers should determine the duration of totality at the several stations with the greatest accuracy possible, so as to afford means of determining the value of the diameter of the Moon that should be used in the prediction of solar eclipses.

The President said that Mr. Evershed had dealt with a subject which did not appear to have previously received the attention it deserved. He hoped that one result of the paper would be that he would be able to arrange with some other observers to take the opposite limit to that which he decided upon.

Mr. Maunder read a paper on "The Proper Exposure to be given to Photographs of the Corona." He said that one of the most interesting features in the observations of the recent eclipse was the enormous variation in the length of the exposures given. Taking *f*/15 as a representative instrument, and supposing that the most rapid plates available were used, four different classes of exposure appeared to be indicated according to the four different regions of which it was desired to obtain a photograph. For the prominences $\frac{1}{60}$ sec. might be taken as normal. For the lower corona ·05 to ·1 sec. seemed to be the most suitable; by prolonging the exposure to $\frac{1}{4}$ or perhaps $\frac{1}{2}$ sec. a great increase of the area was commanded; and one second might be taken as marking almost the limit of really useful exposure for the corona as a whole. If beyond this it was desired to secure the long rays, observers must be prepared to expose for 100 seconds or upwards, sacrificing in this case the inner corona. It was possible that they might find exposures of 200 or 300 seconds, or even longer, advantageous.

Remarks were made on the paper by Mr. Thwaites, Mr. Evershed, and Mr. Wesley. In reply, Mr. Maunder said that if he were fortunate enough to go to the eclipse of May 1900, he should give instantaneous exposures, and also expose for the whole of totality, but should make no use of lengths between these.

Mr. Chambers gave a summary of a paper on travelling to and in Portugal and Spain, with reference to the eclipse of the year

1900. Mr. Chambers was strongly in favour of the various sea-routes in preference to railway travelling.

Capt. Noble said that unless Spain had greatly altered since he knew anything of it, it would be quite out of the question to take ladies, except to one or two of the larger towns.

Mr. G. Knowles said that he did not think the manners were as bad in Portugal as Capt. Noble implied. He recommended Ovar as a town where good accommodation could be found.

Mr. Maunder added that there would be no difficulty about Algiers, which was easily reached from both France and England, and the hotel accommodation was no doubt very good.

ROYAL METEOROLOGICAL SOCIETY.

THE Monthly Meeting of the Society was held on Wednesday evening, April 19th, at the Institution of Civil Engineers, Westminster. *Mr. F. C. Bayard, LL.M.*, President, in the Chair.

Mr. H. Mellish read a paper on "Soil Temperature," in which he discussed the observations from the thermometers at various depths in the soil which have been made at the stations of the Royal Meteorological Society. The records have been carried on at many of the stations since 1881, and observations have been made at the following depths in the soil—3 inches, 6 inches, 1 foot, 2 feet, and 4 feet. It appears that in nearly all cases the annual temperature of the soil at a depth of 1 foot is slightly higher than that of the air. In winter time the air and the soil at 1 foot have about the same temperature, the soil being often a little warmer till about the end of January, after which for the next two months the air has a small advantage; but in the summer months the soil at one foot is generally warmer than the air, the difference exceeding 3° at several stations. Mr. Mellish shows that on the mean for the year the light soils are $1^{\circ}\cdot 0$ warmer than the air, while the strong ones are only $0^{\circ}\cdot 2$ warmer, and he is of opinion that near the surface we may expect to find wider extremes of temperature in light soils than in strong ones, but that the heavier soils are better conductors of heat, and that consequently the extremes are propagated to greater depths in heavy soils than in light ones.

A paper "On some Phenomena connected with the Vertical Circulation of our Atmosphere," by *Major-General H. Schaw*, C.B., R.E., was read by the *Secretary*. The author has for some time past been studying the circulation of the atmosphere over Australasia, and in this paper gives the results of his examination of the weather charts, chiefly in regard to the interaction of cyclones and anticyclones upon each other.

Rotation Period of Mars.

In 1869 February I frequently observed Mars with a 4¼-inch refractor and noted on several nights the time when the Syrtis Major (Kaiser Sea of Proctor and Green) was apparently central on the disc. One of the best observations was on February 4, when I made a drawing representing the object named in mid-transit at 11^h. In 1884 February, when Mars was similarly situated with respect to the Earth, I obtained some excellent views with a 10-inch reflector, power 252, and noted the Syrtis Major central on February 14, at $5^h\ 55^m$; February 15, $6^h\ 35^m$; February 19, $9^h\ 5^m$; and February 22, $11^h\ 4^m$. Now that Mars, after another interval of 15 years, has been recently presented under the same circumstances, I have secured some further transits of the object referred to, the latest of which was on March 7, $8^h\ 31^m$.

Though the rotation period of Mars has been accurately determined by several computers, I have been comparing my various observations during the past 30 years to find what sort of correspondence they would offer with other results. The whole period between 1869 February 4 11^h and 1899 March 7 $8^h\ 31^m$ comprises 10,987 days, 21 hours, 31 minutes, during which Mars has performed 10,710 rotations. The mean period of rotation during this interval was

$$24^h\ 37^m\ 22^s{\cdot}70.$$

This value comes between those of Proctor and Bakhuyzen. But for the sake of comparison it may be interesting to quote the summary of previous determinations given in my 'Telescopic Work':—

Year of Publication.	Observer or authority.	Rotation period.		
		h	m	s
1666	J. D. Cassini	24	40	0
1704	J. P. Maraldi	24	39	0
1781	W. Herschel	24	39	21·7
1784	W. Herschel	24	37	27
1838	J. H. Mädler	24	37	23·8
1845	O. M. Mitchell	24	37	20·6
1859	A. Secchi	24	37	35
1864	F. Kaiser	24	37	22·62
1866	R. Wolf	24	37	22·9
1869	R. A. Proctor	24	37	22·735
1873	F. Kaiser	24	37	22·591
1873	J. F. J. Schmidt	24	37	22·57
1883	A. Marth	24	37	22·626
1885	H. G. v. de S. Bakhuyzen	24	37	22·66

Kaiser's, Proctor's, Marth's, and Bakhuyzen's are based on very long intervals of time, modern observations having been compared with ancient ones by Huygens in 1659, and Hooke and Cassini in 1666, &c. The agreement in the various results is good, but when the length of the periods and the large number of rotations performed are considered, it is perhaps surprising that the accordances in the four instances alluded to are not closer. The inference is that certain objects figured by the old observers were not, in all cases, the prototypes of certain conspicuous forms seen in recent years. Quite possibly physical changes have materially affected the relative prominence of some of the markings.

It will be observed that the three earliest values in the list exhibit a discordance of two minutes or more when compared with the rest. This may seem curious when we reflect that, observationally, Cassini, Maraldi, and W. Herschel were giants in their day. But it is easily explained on the assumption that they gave the *observed* periods of rotation instead of the corrected periods. Mars apparently loses one rotation during a revolution round the Sun, so that an average minus correction of more than two minutes has to be applied to the observed time of rotation. For very short periods, of course, the correction may be essentially different, and varies quickly, depending as it does upon the inconsistent mutual positions of the Earth and Mars.

Bristol, 1899, March 14. W. F. DENNING.

A remarkable Solar Outburst.

THE most remarkable solar outburst that it has ever been my good fortune to witness occurred on the morning of March 16, at 11 A.M. Greenwich time. I was examining the leading portion of the fine group which came round on the 15th, and was then a few degrees inside the east limb, and which leading portion then (11 A.M. on the 16th) consisted of a northern, a western, and a southern spot, each crossed in a more or less north-and-south direction by a bright faculous bridge. One or two minutes afterwards I noticed a cyclonic motion in the north-eastern portion of the northern spot, a bright faculous tongue shooting along the centre of the penumbra in an easterly and southerly direction. At about 11.3 A.M. a shaded area appeared to the east of these three spots, forming with them a rough quadrangle. This shaded area rapidly became darker, and within two or three minutes was a heaving and eddying mass of bright and dark spots, appearing and disappearing with lightning rapidity, and defying adequate description. Quite suddenly a large portion on the north cleared away, revealing, beneath, a dark umbra crossed by one or two brilliant serrated bridges, the southern portion being as yet uncovered. To my dismay clouds then hid the Sun from view.

When I was again able to observe it, at G.T. 12.18, the area in question had resolved itself into a spot consisting of an umbra on its north edge, and penumbra on the west, south, and east, the whole being crossed from north to south by a brilliant branching faculous bridge, and a faculous tongue projected from the north edge of the spot, partly over the umbra to the west of the bridge. The northern spot had in the interval of 1¼ hour been

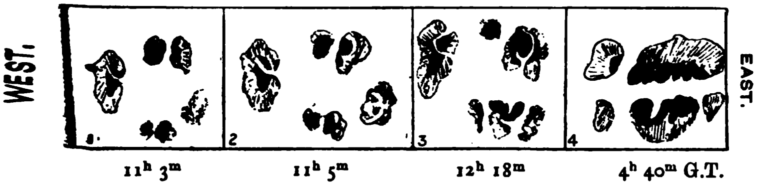
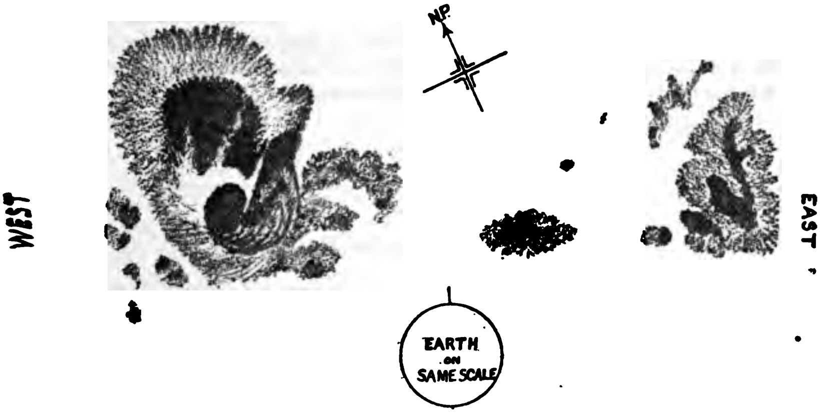


Diagram showing the formation of the leading spot on March 16.



The whole group as it appeared on 1899, March 18, 10h 48m till 11h 8m G.T.

spanned by a narrow, curved, and branching faculous bridge, conspicuous changes having also taken place in the western spot. Although the force of the outbreak was now evidently greatly weakened, rapid changes were still in progress in all the spots. At 4.40 P.M. the four spots had resolved themselves into one large spot crossed by a faculous bridge from east to west, and also by another bridge from north to south intersecting the first, which two, together with a minor bridge on the south-east, divided the spot into five portions. At 2.28 P.M. on the following day (17th) the spot was almost unrecognizable, although the irregular cruciform bridge remained. At G.T. 10.48 A.M. this morning (18th) the spot had completely altered in appearance, violent cyclonic motion being very apparent in its southern half.

One point of great interest during the outbreak at, and just after, 11.3 A.M. on the 16th, was that the faculous matter did not appear to be driven aside by an outrush of vapour, nor to be drawn downward by a downrush, but rather it appeared to dissolve and recondense with great rapidity over and over again, disappearing finally by dissolution, much as clouds do in our own atmosphere.

It is, perhaps, worthy of remark that the *preceding* end of this fine group is only about 8° or 10° east of the longitude of the *following* end of the great group of September last, while the northern limit of the latter about corresponded in latitude to the southern limit of the present group. On February 21 last a single very small spot appeared in the position of this group, which on the 22nd had increased to seven or eight small spots, only three of which were visible on the 23rd; while on February 24, and until this region passed round the western limb, I did not detect any further disturbance in this position.

The foregoing observations were made with a 5-inch refractor and a power of 210, the Sun's image being projected upon the base of a darkened box attached to the eye-end of the telescope, giving a scale of about 1 inch to 1 minute of arc.

Madeira, 1899, March 18. WM. ANDERSON.

Helium in Long-Period Variables.

VERY little is known about the visual spectra of most of these stars; but, judging from the scanty observations so far made upon them, they seem to show considerable variety. The easy-going rule, *ex uno disce omnes*, is plainly inapplicable; each demands, but only one—that of Mira Ceti—has yet received, separate and detailed study. To be completely profitable it should be accompanied by simultaneous photographic research: the spectral map needs to be viewed as a whole; its sections are mutually explanatory; taken apart they very imperfectly disclose their meaning. Interpretation, here as elsewhere, is intimately dependent upon correlation. The question of helium, above all, is one as to which two and two want putting together; and at present the evidence bearing upon the presence of that wonderful substance in "long-period variables" is merely fragmentary. In some, however, the brightening of the characteristic yellow ray is unmistakable, although its emergences are brief, and appear somewhat capricious. The following stars will serve as examples. Their number might doubtless be indefinitely augmented if D_3 were looked for when F begins to shine.

Name of Star.	Observer.	Date.
R Cygni	Maunder	Sept. 21, 1888.
χ Cygni	Espin; Taylor	May 1889.
R Andromedæ	Espin	Sept. 25, 1889.
R Leonis	Gruss and Laska	May 6, 1894.
R Aquilæ	Gruss and Laska	June 23, July 4, 1894.
R Coronæ	Espin (doubtful)	April 1893.
R Cassiopeiæ	Espin (suspected)	Sept. 35, 1889.
S Cassiopeiæ	Espin (suggested)	Nov. 27, 1889.

We can see, then, that the usual emissive association of hydrogen with helium is at least partially maintained in this class of spectrum. Noteworthy, too, is the circumstance that, in all the stars cited, $H\beta$ literally blazes near maximum, and in some $H\alpha$ as well; so that the mode of hydrogen-radiation in them is markedly different from what it is in Mira. That D_3 is not isolated in their spectra may safely be affirmed; but it remains to be seen whether its companions are dark or bright. Mixed helium series, the bright members being *always* the least refrangible of each set, are quite common in other stellar families. Among Wolf-Rayet stars, γ Argûs exemplifies the peculiarity; and it is conspicuous in P Cygni and its congeners.

In the spectrum of Mira no golden ray has been—although it may yet be—detected; but dark-line coincidences with helium rays in the blue and green arrest attention, and should repay further enquiry. Thus, the solitary representative of the second principal series at λ 3889 (the leader of the "parhelium" company, as it might be called) was seemingly photographed at Potsdam in 1896 as a dark band in contact and contrast with a brilliant $H\zeta$, none of the remaining hydrogen-lines being similarly accompanied. Another prominent absorption-ray, marked by Dr. Vogel as non-solar, is situated at λ 4009, just in the place of a familiar "Orion"-line, a member of one of the subordinate "parhelium" series. Moreover, the helium-lines at λ 4143, λ 4169, λ 4472, λ 4713, λ 4923, and λ 5016 were to all appearance measured as elements of absorption on the Stonyhurst plates taken at the ensuing maximum of November 1897, although their identity cannot, on their evidence alone, be regarded as fully established *. Comparisons with future spectrograms of such stars as χ Cygni and R Leonis will perhaps serve this interesting and important purpose.

A. M. Clerke.

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* Father Sidgreaves kindly permits me to add, in anticipation of what he may himself publish on the subject, that the wave-lengths given by him in the 'Monthly Notices' for April need a correction to the fourth figure of

+1 between λ 4400 and λ 4440,
+2 „ 4440 „ 4670,
+1 „ 4670 „ 4760.

Hence, a strong dark band placed at λ 4470 proves to be really situated at λ 4472. It accordingly coincides with the most prominent of the helium-lines in Rigel, Bellatrix, β Crucis, &c.

Perturbations of the Leonids *.

When the present investigation was undertaken, our knowledge of the perturbations of the Leonids was due to an investigation carried on thirty years ago by Professor J. C. Adams.

His object was to compute the shift in the nodes of the meteoric orbit due to perturbations, and to compare the calculated amount with the amount which had been deduced by Professor Hubert A. Newton from observations made at intervals during the last 1000 years.

For Professor Adams's purpose the perturbations to be computed were the average perturbations; and he accordingly employed Gauss's method, in which the mass of the disturbing planet is supposed to be distributed round its orbit in quantities proportional to the time that the planet occupies in travelling over each portion of its course. This elegant method furnishes the average amount of each perturbation on the supposition that the periodic times of the disturbed body and of the disturbing planet are incommensurable, so that in the course of time the two bodies present themselves in every possible position to one another.

This condition, however, has been but imperfectly fulfilled within the limited period of 1000 years over which the recorded observations extend, especially in the case of the three planets which influence the Leonids most, and indeed are almost the only planets whose attraction needs to be taken into account. These are Jupiter, Saturn, and Uranus. A comparison of the periodic times shows that fourteen revolutions of Jupiter approximate in duration, within about one-fifth of a year, to five revolutions of the meteors; two revolutions of Uranus occupy about one and three-quarters of a year more than this same time, and nine revolutions of Saturn correspond within a fraction of a year to eight revolutious of the meteors.

These cycles have been several times repeated within the period over which the observations extend; and one consequence of these cycles is that there have been oscillations in the rate of the advance of the node about its mean value, so that the times for the showers assigned by applying to the orbit the average shift of its node have usually differed by several hours from the actual times. On one occasion—in A.D. 1533—the shower anticipated the computed time by about twenty-six hours, and, as the present investigation shows, a deviation of comparable amount and in the opposite direction is to be expected this year. Accordingly, even if our sole object were to enable astronomers in future to predict more satisfactorily the times of the greater Leonid showers, it would be necessary to prepare for the task by first studying the actual amount of the perturbations in each revolution, and, moreover, for meteors occupying various stations along the stream.

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* Abstract of a paper by G. Johnstone Stoney and A. M. W. Downing, read before the Royal Society, 1899, March 2.

For, in fact, the perturbations have not only differed in different revolutions, but even within a single revolution, the meteors which occupy successive positions in the procession are differently affected by the surrounding planets, as is confirmed by the definite results which Herr Berberich has obtained by assuming successively two epochs for the perihelion passage. The dense part of the stream, with which we are chiefly concerned, and which we may call the ortho-stream *, is now so long that it takes between two and three years to pass each point in its orbit, so that the configurations in which the several parts are presented to the disturbing planets are markedly different. Accordingly, perturbations must have produced in this long stream both sinuosities and an unequal distribution of density †; and the first step towards increasing our acquaintance with these and other kindred phenomena, as well as towards gaining a better insight into the past history of the swarm, is to aim first at securing a more intimate knowledge of the perturbations.

With this end in view it was decided, as a first step, to compute the actual perturbations of a definite part of the stream over the whole of one revolution, taking that part of the ortho-stream of which Adams had determined the orbit, and extending the computation over the revolution from the date of the great shower of November, 1866, until that day in January, 1900, when the same part of the stream will return to the Earth's orbit.

Adams's calculation was based on determinations of the radiant-point which were made in 1866, before photography had lent the aid to astronomy which it now yields. Moreover, the circumstance that the Earth deflected the meteors which were then observed by an amount which varied as the shower progressed, was not at that time attended to by observers. Owing to these imperfections, there is a considerable probable error in the mean of the determinations which were made in 1866, and a corresponding uncertainty in the values of the elements computed from that mean. We are accordingly only justified in employing Adams's orbit as approximate. But, fortunately, an error in the orbit, of such an amount as is at all likely to exist, will not materially affect the perturbations of the orbit, which are what we have at present in view.

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* In order to facilitate the study of the Leonids it is convenient to distinguish between a great body of them—the ortho-Leonids—which are travelling round the Sun in nearly identical orbits, and another class of Leonids which we may call clino-Leonids, that are pursuing courses which differ in a more considerable degree from the ortho-orbit. By the ortho-orbit is to be understood the mean of the orbits of the ortho-Leonids.

The ortho-Leonids at present form a compact stream of such a length that it takes nearly three years to pass each point of its orbit, and so narrow that when the Earth passes obliquely through it the transit occupies only some five or six hours; whereas the clino-Leonids form a less dense and wider stream, which has spread itself the whole way round the ring, and which produces in every November, when the Earth passes through it, a feeble meteoric shower that lasts for several days.

† One consequence of the existence of irregularities in the stream of ortho-Leonids is that the ortho-orbit at one cross-section of the stream (*i. e.*, the mean of the orbits of the meteors occupying that situation in the stream) is in general not absolutely identical with the ortho-orbits at other cross-sections.

The main stream of Leonids—the ortho-stream—is narrow and very long, and it is convenient to divide it into segments, each of which shall be of moderate length. Through one of these, which we may call segment A, the Earth passed in November, 1866, and on that occasion there was withdrawn from it that small portion which consisted of meteors which either encountered or passed close to the Earth. Those that actually plunged into the Earth's atmosphere were destroyed; those that passed near were deflected, and were also either accelerated or retarded, and they thus become clino-Leonids. It is with the great majority of the meteors in segment A, which escaped both these fates and continued to be ortho-Leonids, that Adams's investigation is concerned. He ascertained their orbit; and starting from the elements of the orbit as determined by him, the actual perturbations which it has since undergone have been computed, and the main results thus arrived at are embodied in a table *.

As already stated, the calculation has been extended over an entire revolution of that portion of the stream which we have called segment A; and in computing the perturbations, account has been taken of the attraction exercised upon these meteors by Mars, Jupiter, Saturn, and Uranus. At first Venus and the Earth were included, but as the influence of these planets was found to be insensible, they were omitted from the latter part of the calculation.

The most noteworthy features are a near approach of segment A to Saturn in April, 1870, and a near approach to Jupiter in August, 1898, at which latter time the meteors of that part of the stream were at a distance from the planet of only 0·9 of the mean radius of the Earth's orbit. The consequences of these near approaches are brought out in the table, which exhibits in detail the effects upon one segment of the stream of the very unusual amount of perturbation which the Leonids have suffered in this revolution. Uranus produced but little effect, as the planet was at a distance when the swarm crossed his orbit, and the influence of Mars was trifling.

The mean distance of the meteors occupying segment A of the stream has been undergoing so much extension, that the meteors will at the end of the revolution find themselves with a periodic time longer by one-third of a year—an amount of change which must largely affect their future history, unless this great perturbation is compensated by what happens elsewhere or at other times.

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* See original memoir, or Ast. Nach. No. 3555.

At the epoch 1899, November 15, the longitude of the node will be 53° 41′·7, a position which the Earth will reach on 1899, November 15^d 18^h. It is probable, therefore, that the middle of the shower of the present year (1899) will occur nearly at this time, since segment A in the stream, for which our calculations have been made, is situated in the stream less than three months' journey of the meteors behind the segment which the Earth will encounter next November, and which we may call segment B. This conclusion, however, rests on two assumptions: (1) That segments A and B were, in 1866, moving in orbits that did not much differ; (2) That the perturbations which segments A and B have since suffered have not much differed. Both assumptions are probable, but unfortunately neither is certain; so that the prediction can only be offered with reservation. If the shower occurs at the time anticipated, it will be visible from both Europe and America.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Washington Star Catalogue.

GENTLEMEN,—

The character of the notice of the Second Washington Star Catalogue, which appeared in the 'Observatory' for March 1899, leads me to suppose that the critic desired to give an accurate account of the Catalogue and a correct interpretation of such portions of the Introduction as seemed to him worthy of attention.

On this assumption I have thought that some notice of certain points raised by the reviewer, as well as the correction of some misapprehensions on his part, would tend to make the catalogue more valuable to those who have occasion to use it, and, at the same time, aid in some degree the progress of practical astronomy.

If the quotation in the first paragraph of the review had referred to the *observations* as well as the *catalogue*, it would have more nearly represented the facts, but, even then, the statement would have been somewhat extravagant. It may be well here to remark that in the *seventh* line of the Preface to the Catalogue the number 39867 should be 52820.

The reasons for stopping the observations for fundamental Right Ascensions have been frequently given in the annual volumes of the Naval Observatory, and are, briefly:—

(1) The lack of a sufficiently good clock.

(2) Lack of knowledge of the absolute personal equations of the various observers.

(3) Too many different, and sometimes untrained, observers.

The *first* and *third* of the above objections have never been eliminated.

The *third*, *fourth*, and *fifth* paragraphs on page 131 of the 'Observatory' refer to the difficulty that seems to have been found in the observations with all Meridian Circles where fundamental work was attempted.

It is true that there are comparatively large differences between the annual results in N.P.D. for the Ephemeris Stars. But these differences are brought out by the custom of reversing the instrument annually; and by these reversions it has been possible to eliminate, practically, such errors from the final results. With a non-reversible instrument, such errors would not have been apparent nor would they have been eliminated.

The discordances between Direct and Reflection observations are somewhat large, but not so large as in other similar and well-known instruments.

The critic probably failed to read the whole article on Reflection-observations, pages xxix to xxxii of the Introduction, or he would have noticed that the method of treating the R—D observations to which he refers, and which produces discontinuity in the corrections at the zenith, was that used in the *original* annual reductions. The method *finally* used for the data in the Catalogue is given near the bottom of page xxx, and is simply using the mean of the two values of $\frac{D-R}{2}$ or $\frac{(Dn-Rn)+(Ds-Rs)}{4}$.

This method is a new departure, and on its validity rests all the North-Polar Distances of the catalogue. The reasons for adopting this method are given at some length in the Introduction, pages xxx to xxxii; and in greater detail in the 'Astronomical Journal,' No. 454, Boston, Jan. 25, 1899.

On pages xxx and xxxii of the Introduction these probable sources of error in the original observations and reductions are enumerated. More space is given to the discussion of the probable tilting of the objective because, so far as I know, it has not been considered, heretofore, as a source of error in reflection observations. Shifting of the sliding tube at the eye-end of the telescope and the lack of definite knowledge of the flexure constants are the other two sources of error. The magnitude of these errors are probably in the *inverse* order in which they are mentioned.

It is not easy to understand what is meant by the reference, on page 132 of the 'Observatory,' to the determination of personal equation for *faint* stars over *bright* threads. In observing the transits of stars from the *seventh* to the *ninth* magnitude on the same setting of the instrument, and over dark threads, the intervals, in time, between the consecutive stars are independent of the personal equation. Let the normal intervals between the *first* and *second*, the *second* and *third*, the *third* and *fourth*, the *fourth* and *fifth*, &c., be observed over dark threads until the probable errors of the results are reduced to the desired magnitude. Then place a wire screen over the objective when the *second* star is observed, the *first* and *third* being observed without the screen. The second star will be reduced in apparent magnitude until it must be observed over bright threads. The observed intervals between the *first* and *second* and between the *second* and *third* stars, when compared with the normal intervals, will give the peculiar personal equation due to a star of that apparent magnitude when observed over *bright* threads. This value has no connection whatever with the ordinary personal equation. My experiments in this class of observations have led me to the conclusion that all transits of faint objects observed over bright threads are wholly untrustworthy unless this peculiar personal equation has been determined for each observer.

With regard to the corrections for equinox, it should be said that the method of computation was adopted primarily to test the relative accuracy of the observations of the Sun; and I am satisfied that the success in that direction was ample compensation for the additional labour involved.

Moreover, I submit that an observation which, because of its date, is not useful in the method which was employed for the data in this catologue is equally undesirable in weighted equations involving the corrections for equinox, obliquity of the ecliptic, and the assumed latitude.

I am not aware of any vital error in Table 17, but if there is one I should be glad to know it.

The above notes on the article in the 'Observatory' are written simply for the purpose of correcting what seemed to be erroneous impressions in regard to the methods used in forming the catalogue and are not in any way an apology for, or a defence of, that publication. The catalogue in question is constructed from the observations with one instrument, whether they be good or bad, and must stand or fall, among astronomers, on the character of its results.

I cannot close these notes without expressing my thorough appreciation of the spirit of courtesy and fairness which seems to pervade the article in the 'Observatory.'

Washington, D.C., April 11, 1899. J. R. EASTMAN.

Eclipses mentioned in Early Roman History.

GENTLEMEN,—

If I have not already tired you by my remarks on ancient eclipses, I should like to be allowed space for the following few more, on two of the Sun and one of the Moon, which purport to be mentioned in Livy.

Sir G. Cornewall Lewis, in his well-known work 'An Historical Survey of the Astronomy of the Ancients,' refers (p. 227) to two of these, but omits the first, probably overlooking it, because, had he regarded the account as not really referring to an eclipse, he would probably have said so, as in the case of a darkness in the daytime mentioned in Livy, xxxviii. 36. Many historians, however, connect the battle of Zama (the final contest of the Second Punic War) with a partial eclipse of the Sun which occurred on the 19th of October, B.C. 202, and in the account of Hannibal in the American Cyclopædia the eclipse is said to have contributed to Scipio's victory. Mr. Bosworth Smith, in his 'Carthage and the Carthaginians,' states (no doubt on account of the eclipse) that the battle was probably fought in October. Mommsen, however, says in a note (book iii. ch. 6, vol. ii. p. 359 of Dickson's translation):—"The time was the spring or summer of the year 552 [*i. e.* A.U.C.]; the fixing of the day as the 19th October, on account of the alleged solar eclipse, is of no account." And he is certainly right; for Livy, in mentioning the report of the eclipse (lib. xxx. c. 38) gives it as one amongst several ("solis Cumis orbis minui visus, et pluit lapideo imbri") prodigies which took place when the truce with Scipio was broken by the Carthaginians, and says that these occurred during the celebration of the Apollinarian games at Rome, which were interrupted by an extraordinary rising of the waters of the Tiber. Now the Apollinarian games (which had been instituted only ten years before that time) were held in the month Quintilis, or, as it was afterwards called, July; and though by the unreformed Roman calendar this would not correspond to our July, it would be much earlier, not later, in the year, and be, in fact, not an autumnal, but a spring month. The battle must have been fought some time afterwards, probably in April or May. A central eclipse of the Sun occurred on the 25th of the former month in that year, but Oppolzer's 'Canon der Finsternisse' shows that this could not have been visible in Europe. Livy's words then must refer to a real or fancied diminution in the Sun's light not due to an eclipse.

Sir G. Cornewall Lewis says that the earliest authentic mention of an eclipse in Roman history relates to that of the Sun on the 14th of March in the year corresponding to B.C. 190. Of this Livy says (lib. xxxvii. c. 4) that it also took place during the Apollinarian games, the fifth day before the ides of Quintilis, when "cœlo sereno interdiu obscurata lux est, cum luna sub orbem solis subisset." I must apologize for two slips in my letter to you of 1888 May ('Observatory,' vol. xi. p. 231), where June 10 appears instead of July 10 as the fifth day before the ides of Quintilis, and "during the life of that historian" should be "during the life of that poet" (meaning Ennius). Lewis remarks that the difference between the time of the eclipse and of the date in Livy affords a measure of the error of the Roman calendar at that time. If so, this would make its reckoning nearly three months too early, about the same as when Julius Cæsar reformed it by inserting intercalary days to about that amount.

It is only on this account that I wish once more to refer to one other eclipse mentioned in Livy, viz. that of the Moon which occurred in B.C. 168, on the eve of the battle of Pydna, between Æmilius Paulus and Perseus, king of Macedonia. This took place, according to the Roman calendar in its then state, on the 3rd (the day before the nones) of September. We know that the eclipse occurred on the 21st of June by the Julian calendar, so that the Roman reckoning would then be about 2½ months too early. But of course it is well known that the pontiffs were very irregular in their reckonings of the calendar arrangements. According to Livy (lib. xliv. c. 37), this eclipse of the Moon was predicted and explained to the soldiers by C. Sulpicius Gallus, a military tribune in the Roman army; and this is corroborated by Polybius, though Cicero and others give a somewhat different account, that Gallus merely quieted the troops when alarmed at the eclipse by instructing them as to how it was produced.

This is probably the last time I shall trouble you on the subject of ancient eclipses, and I remain,

Yours faithfully,
Blackheath, 1899, April 3. W. T. Lynn.

P.S.—It has been thought that the eclipse of B.C. 217, February 11 (which was total in Northern Africa, and was probably visible as a partial eclipse in Italy), is alluded to in Livy, lib. xxii. c. 1, but this is very doubtful. It occurred about two months before the battle at Lake Trasymene; and the expression used by the historian is the same as that quoted above with reference to a diminution in the apparent size of the Sun reported to have taken place (with other prodigies) in the year B.C. 202, before the battle of Zama. The latter could not, as we have shown, refer to an eclipse, and it is scarcely likely that the former does. If not, only one eclipse of the Sun is mentioned in Livy.—W. T. L.

PUBLICATIONS.

The Total Solar Eclipse of 1898 January 22 *.—In a well-printed and popularly-written volume of 104 pages, Father de Campigneulles gives an account of the observations of his party at Dumraon, close to the station selected by the Survey of India Expedition. The Jesuit party numbered 19, including local volunteers, and was equipped with three cameras for photographing the corona, a prismatic and a grating camera for spectroscopic observations, and other instruments. Their limited means compelled them to rely almost entirely on their own resources for the construction and adjustment of instruments. They were without a driving-clock, so their exposures on the corona were

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* 'Observations taken at Dumraon, Behar, India, during the Eclipse of the 22nd January, 1898, by a party of Jesuit Fathers of the Western Bengal Mission.' By Rev. V. de Campigneulles, S.J. London: Longmans & Co.

necessarily short — from instantaneous to five seconds. The duration of totality was found at Dumraon to be less than predicted, the Sun re-appearing a few seconds before the computed time.

Father de Campigneulles gives in this book not only the observations of his own party, but also a summary of the results of other observations. He also gives a review of solar physics in connection with eclipse work, though of course, in the narrow limits of a chapter, he can do no more than very briefly summarize the various theories relating to the photosphere, chromosphere, prominences, and corona. In his description of the corona, exception must be taken to the statement that the streamers show slight variations in size and brightness during an eclipse, and sometimes "a slow but perceptible shifting round the disc." Justification for this view may, no doubt, be found in drawings and reports of visual observations, but it is utterly negatived by the photographic evidence, on which alone reliance is to be placed. It is also unfortunate that the author should attribute the discovery of the spectroscopic method of viewing the prominences without an eclipse to M. Janssen and Sir Norman Lockyer, without mention of the name of Sir William Huggins.

Turning to the report itself, we find a clearly-written account of the results of observations, photographic and spectroscopic, as well as of shadow-bands (very faintly seen at Dumraon), temperature, and miscellaneous observations. The observations were entirely successful, and a good series of corona photographs were taken; but the spectroscopic observations were somewhat less satisfactory, owing to the shortness of exposure necessitated by the want of clock-work motion. Two plates are devoted to reproductions of spectrum-photographs, and nine plates are enlargements from negatives of the corona. Unfortunately, the latter reproductions are disappointing; they appear over-enlarged, and give but a poor idea of the detail on the original plates. Plates 5 and 6 are from negatives taken at the time of first internal contact, and show a ring of light surrounding the remaining portion of the disappearing solar crescent (or chromosphere), which the author seems to consider due to something more than mere "instrumental reflection," but it looks like an ordinary halation ring. Plate 11, fig. 1, shows an extension of the corona of nearly two lunar diameters, but is said to have been taken ten seconds after end of totality. May there not, however, have been a slight error in the time? Plate 11, fig. 2, is a photograph taken 13 seconds after end of totality; it shows a reversed image of the reappearing crescent, and a mere narrow fringe of corona. It seems improbable that an interval of only three seconds could make so great a difference.

Father de Campigneulles' book is extremely readable, and the observations are especially creditable to a party so slenderly provided with instrumental means. W. H. W.

NOTES.

Comet Notes.—Swift's Comet (*a* 1899) promises to be a very conspicuous object in May, being visible in the early morning at the beginning of the month, and throughout the night at the end of the month.

New elements and ephemeris of this Comet by Prof. Kreutz (Ast. Nach. 3556):—

T	1899 April 13·0148 Berlin M.T.	
ω	8° 48′ 52″	1899·0.
☊	25 0 55	1899·0.
i	146 15 48	1899·0.
log *q*....	9·51311	

Ephemeris for Berlin midnight.

	R.A. h	m	s	N. Dec. °	′		R.A. h	m	s	N. Dec. °	′
May 2....	0	5	5	23	26	May 18....	22	34	21	43	43
3....	0	1	28	24	21	19....	22	23	37	45	23
4....	23	57	45	25	18	20....	22	11	40	47	4
5....	23	53	54	26	17	21....	21	58	20	48	44
6....	23	49	53	27	19	22....	21	43	29	50	23
7....	23	45	41	28	24	23....	21	26	59	51	56
8....	23	41	17	29	31	24....	21	8	40	53	24
9....	23	36	40	30	42	25....	20	48	31	54	42
10....	23	31	45	31	56	26....	20	26	33	55	47
11....	23	26	28	33	13	27....	20	3	7	56	38
12....	23	20	48	34	33	28....	19	38	21	57	11
13....	23	14	41	35	57	29....	19	12	56	57	26
14....	23	8	2	37	24	30....	18	47	26	57	20
15....	23	0	46	38	54	31....	18	22	26	56	56
16....	22	52	48	40	28	June 1....	17	51	35	56	13
17....	22	44	2	42	4	2....	17	36	8	55	14

Search ephemeris for Tempel's Second Periodic Comet for Paris midnight, by M. L. Schulhof (Ast. Nach. 3554):—

	R.A. h	m	S. Dec. °		R.A. h	m	S. Dec. °
May 4......	18	49	4·7	June 5......	19	40	4·1
12......	19	2	4·2	13......	19	52	4·9
20......	19	15	3·9	21......	20	3	6·4
28......	19	28	3·8				

The comet even now is brighter than when first seen in 1894, and by the end of June it will be ten times as bright.

Ephemeris of Chase's Comet (1898 VIII.) for Greenwich midnight (Ast. Nach. 3555):—

	R.A.			N. Dec.			R.A.			N. Dec.	
	h	m	s	°	′		h	m	s	°	′
Apr. 27....	10	47	33	35	5	May 13....	10	55	1	32	53
May 5....	10	50	50	34	1	21....	11	0	0	31	42

Tuttle's Comet is now too near the Sun for observation. The following improved ephemeris, by J. Rahts, for Berlin midnight, may be of use to southern observers (Ast. Nach. 3555):—

	R.A.			S. Dec.			R.A.			S. Dec.	
	h	m	s	°	′		h	m	s	°	′
June 9....	6	58	48	8	18	July 9....	8	47	26	22	45
14....	7	16	18	10	52	14....	9	6	14	24	50
19....	7	34	3	13	23	19....	9	25	6	26	47
24....	7	51	32	15	51	24....	9	43	58	28	36
29....	8	10	17	18	15	29....	10	2	45	30	16
July 4....	8	28	46	20	33	Aug. 3....	10	21	25	31	48

Mr. Tebbutt continued his splendid series of observations of Coddington's Comet into the present year; the number of nights on which he obtained observations exceeded 100. A. C. D. C.

MINOR PLANET NOTES.—Two more planets have been discovered:—EL, by Coggia, at Marseilles on March 31; EM, by Witt, at Urania Observatory, Berlin, on April 5.

Planet EK, discovered by Palisa on March 9, has been named Slatin.

Planet 366 (1893 W, Charlois), has been named Vincentina.

A. C. D. C.

THE NEW SATELLITE OF SATURN.—More definite information as to the new satellite has been received from Prof. E. C. Pickering in 'Harvard College Circular' No. 43. A careful examination was made by Prof. W. H. Pickering of two plates taken on 1898, August 16 and 18, and a faint object was found which appeared in different positions on the two plates. The same object is shown on two other plates, taken August 16 and 17, 1898, with exposures of 60^m and 122^m respectively. The position is nearly the same on the two plates taken August 16, but on August 17 it followed this position 33″, and was south 19″, while on August 18 it followed 72″, south 43″. Its motion was direct, and less than that of Saturn, though nearly in the same direction. It cannot, therefore, be an asteroid, but must be either a satellite of Saturn or a more distant outside planet. The proximity of Saturn renders the first supposition much more probable. On August 17, the position-angle from Saturn was 106°, and the distance 1480″. Assuming that it was at elongation, and that its orbit is circular, its period would be 400 days, or five times that of Iapetus. It was at first identified with a very faint object found on plates taken in 1897, and the period of seventeen months was derived from them. This supposition has not been confirmed.

Measurements of the positions of the images give additional material for determining the form of the orbit. The uncorrected positions of the four images referred to the first plate of August 16 as an origin, are for x, 0″·0, +1″·2, +33″·6, and +71″·8; for y, 0″·0, −1″·7, −19″·8, and −42″·1; the corresponding Greenwich mean times are $12^h\ 16^m$, $14^h\ 18^m$, $12^h\ 56^m$, and $13^h\ 12^m$. Correcting for the motion of Saturn, the relative motion with reference to that body is in x, 0″·0, −2″·4, −10″·7, and −22″·0; in y, 0″·0, +0″·1, +2″·4, and +2″·9. It appears from this, that the apparent motion is about 10″·4 a day, at a distance of 1480″. A computation shows that if the orbit is circular, the period must be either 4200 or 490 days, according as the satellite is near conjunction or elongation. These values may be greatly altered if the orbit is elliptical. Since the interval of time between the first and last photographs on which the satellite appears is only two days, it is impossible to predict its position with accuracy. It is probable that its position-angle from Saturn now lies between 280° and 290°, and its distance between 20′ and 30′. These uncertainties will probably be greatly diminished from measures of plates of Saturn taken in Arequipa on September 15, 16, and 17, 1898, which for some unexplained reason have not yet been received in Cambridge.

The direction of the motion, which is nearly towards Saturn, shows that the apparent orbit is a very elongated ellipse, and that it lies nearly in the plane of the ecliptic. Professor Asaph Hall has pointed out that this is to be expected in a body so distant from Saturn. The attraction of the latter only slightly exceeds that of the Sun. Hyperion appears as a conspicuous object on all four of the plates, and the new satellite appears about a magnitude and a half fainter on each. The approximate magnitude is therefore about 15·5. As seen from Saturn, it would appear as a faint star of about the sixth magnitude. Assuming that its reflecting power is the same as that of Titan, its diameter would be about two hundred miles. It will, therefore, be noticed that while it is probably the faintest body yet found in the solar system, it is also the largest discovered since the inner satellites of Uranus in 1851. The last discovery of a satellite of Saturn was made in September 1848 by Professor William C. Bond, then Director of this Observatory, and his son, Professor George P. Bond. The satellite Hyperion was seen by the son on September 16 and 18, but its true character was first recognized on September 19, when its position was measured by both father and son. Soon after, it was discovered independently by Lassell at Liverpool.

Professor William H. Pickering, as the discoverer, suggests that the name Phoebe, a sister of Saturn, be given to the new satellite.

Three of the satellites, Tethys, Dione, and Rhea, have already been named for Saturn's sisters, and two, Hyperion and Iapetus, for his brothers.

THE ASTRONOMICAL AND PHYSICAL SOCIETY OF TORONTO.—The book of Transactions of this Canadian Society is always welcome. We were glad to receive recently the volume for the year 1898, and we found the first article to be of great interest, for it is the next chapter of Mr. Lindsay's "History of the British Nautical Almanac," which he began in the year 1895. He has been fortunate enough to obtain the help of Mrs. Story-Maskelyne, who transcribed notes from the diary and memoranda of her husband's illustrious grandfather, thus enabling Mr. Lindsay to give a chapter on Dr. Maskelyne's assistants, which contains a list, possibly a complete one, of the members of the Greenwich staff between the years 1765 and 1799. We need scarcely say how valuable such a list is, and how convenient to have it printed so as to be readily accessible. There is also a list of the computers and comparers of the 'Nautical Almanac,' of a period earlier than that dealt with in Mr. Dunkin's article recently published in these pages. Interesting facts are given about several of these persons. We note that in those days two of the staff of the 'Nautical Almanac,' George Witchell and William Wales, were both F.R.S., but that the yearly salary of each was only about £70. Another computer, Richard Dunthorne, the son of a gardener in Huntingdonshire, was *butler* to Dr. Long, one of the Admiralty Board, and an intimate friend of the Lowndean Professor. In writing of Brinkley, who was one of Maskelyne's assistants, and afterwards Astronomer Royal for Ireland, Mr. Lindsay remarks that Dr. Meredith, the Vice-President of the Toronto Society, remembers him well; but we learn with regret that Dr. Meredith has died since Mr. Lindsay wrote this, and remembering that Brinkley came to Greenwich in 1787, we cannot help thinking what a long link with the past is here broken.

Among the other memoirs in the volume there is little of original work in astronomy. There is a long paper by Mr. Lumsden on the establishment of a public observatory, in which he describes what has been done in this way in other places; there does not seem to be a near prospect of the establishment of such an institution in Canada, and in our opinion this is scarcely to be regretted. The President of this Society gives an annual address on the progress of astronomy, and that delivered by Mr. Arthur Hervey on 1899, January 25, was no less scholarly than those by his predecessors. We cannot refrain from quoting a few sentences in which he divides the members of his Society into two classes:—"We have the proselytising section, who desire to go out into lanes and by-ways and invite their fellow men to join them in admiring the features of the Sun and Moon ..... We also have those who, having gained some insight into the first principles of astronomy and physics, wish to get further knowledge for themselves, and find in meeting other like-minded people incitement and encouragement in this patient and pleasurable work. These members refuse to think that the cause of science is served by flaunting her achievements and chanting her praises after the manner in which the attractions of a circus are paraded." Let us assure our Canadian friends that the first-mentioned section is not unknown on this side the Atlantic; and that the proselytising is not unconnected with the practice of somewhat unwarrantable mutual admiration.

M. Darboux, *doyen* of the Paris Faculty of Sciences, and Prof. Lippmann will represent Paris University at Sir Gabriel Stokes's Jubilee at Cambridge, in June next.

The Watson Medal of the American National Academy of Sciences has been awarded to Dr. David Gill, H.M. Astronomer, Cape of Good Hope.

It is announced that a Russian expedition is about to proceed to Spitzbergen to measure arcs of the meridian in that region.

Lord Rayleigh and Prof. G. H. Darwin have been elected honorary members of the New York Academy of Sciences.

The Fiftieth Anniversary of the founding of the St. Petersburg Physical Academy was celebrated on April 13.

Visitation-Day at Greenwich will be on Saturday, June 3. The Dinner, as in recent years, will be held at the Criterion Restaurant, Piccadilly Circus.

From an Oxford Note-Book.

In 'Astronomical Journal,' No. 459, is a long series of observations of Comet 1898 I. by C. D. Perrine. The comet started in declination +23° and ran northwards to +56°, then back to +48°. The 142 comparison stars are in many cases taken from the corresponding volumes of the Catalogue of the *Astronomische Gesellschaft*; and I looked with interest for those in the zones observed at Cambridge, which we have been photographically revising at Oxford. There are only three such, nos. 3, 4, and 5 in the list, in zones +26° and +27°; but I was puzzled to find them attributed to *Rogers*, Camb. A.G. Catal. 13050, &c.

The observations at Cambridge were made and reduced by that venerable astronomer, Mr. A. Graham, as most people know, and the places given are those of his Catalogue. Looking further down the list, I saw the reason of the error. In zones +52° to +54° there are a good many stars taken from the observations made at Harvard by Prof. W. A. Rogers, and unfortunately these are described as being made at Cambridge, without the qualification (Mass.) or (U.S.). So that Mr. Graham's observations have been, by some hasty slip, included under the same head. The mistake as it stands is a trivial one in some aspects. It does not affect the R.A. or Dec. of the comet, and no doubt Mr. Graham's reputation will not materially suffer from the temporary loss of credit for three stars. But I venture to think that the original cause of the mistake is a matter on which most Englishmen, and I should hope most Americans, would feel strongly. Can any good reason be shown for usurping an honoured name and introducing confusion of this kind? I imagine those who originally gave the name Cambridge to a suburb of Boston did so in reverence for Cambridge; and would have hesitated to do so had they foreseen that the reputation of the godchild would grow so fast that it might be confused with its god-parent. That the newer reputation has waxed so prosperously is a matter for congratulation to all concerned, and this is not the place, nor have I the intention, to discuss the relative achievements of the two places. But if there is danger of mistakes such as that I have quoted, would it not be a graceful act for the younger institution to give up the misleading name? It is, apparently, not sufficient to attach (Mass.) or (U.S.) to the name, for the suffixes get left out. And then it excites some people with no feelings to write Cambridge (Eng.)!! Do not Americans resent this? Americans are careful for the preservation of Shakspeare's birthplace, even to the erection of drinking-fountains therein: will not American astronomers save the name of Newton's home from indignity?

I do not quite know who is responsible for christening the Harvard portion of the A.G. Catalogue. Prof. Pickering is generally careful to use the name Harvard throughout—'Harvard Annals,' 'Harvard Photometry,' &c. The Zone-Catalogue is printed as vol. xv. of the 'Harvard Annals,' though the pages are all headed Cambridge, U.S. Of course there is astronomical work done in Cambridge (Mass.) which is not done within the precincts of Harvard University; for instance, the 'Astronomical Journal' was edited by Dr. Gould, and is edited by Mr. Chandler, in Cambridge (Mass.). It is, I am well aware, difficult to say what name should be thereto attached other than Cambridge (Mass.) or Cambridge (U.S): I do not think it becomes us to make a definite suggestion on the point; but I do earnestly hope that the evil will be remedied before it becomes too late to avoid continual confusion. I notice this morning a similar confusion in the *Astr. Nachrichten*, No. 3561, p. 174, in connection with observations of the same comet.

AMONG the verses of Christina Rossetti I came across the following:—

> The half-moon shows a face of plaintive sweetness,
> Ready and poised to wax or wane;
> A fire of pale desire in incompleteness,
> Tending to pleasure or to pain:—
> Lo, while we gaze she rolleth on in fleetness
> To perfect loss or perfect gain.

The idea is a beautiful one and beautifully used in the next verse; but is it quite fair on the half-moon? The poetess seems to think the waxing half-moon and the waning half-moon to be momentarily indistinguishable, as though we had instantaneous photographs of a pendulum at its lowest point when swinging in reverse directions. Perhaps I do her an injustice, but this is the impression her verse gives me. Had she realized the continuity of the Moon's changes I do not think she would have taken the half-moon as the symbol of oscillatory changes which might be either in one direction or the other.

A CLEVERER scientific allusion is made by another poetess in representing the troubles of a poet:—

> And because I was a poet, and because the public praised me,
> With a critical deduction for the modern writer's fault,
> I could sit at rich men's tables—though the courtesies that raised me,
> Still suggested clear between us the pale *spectrum of the salt.*[*]

The use of the "spectrum of the salt," with the vague suggestion that it may represent the absent salt as a pale spectre may represent an absent human being, is a delightful quip; but what irony of fate that the "pale spectrum" in question should chance to be the brilliant sodium lines!

CLIPPINGS from examination papers:—

The Milky Way is bifurcated, one part going south and the other north, and then it extends away to such an infinite distance that we gradually lose sight of it altogether. . . . In the constellation of Taurus is to be found the Coal Sac, which was so named on account of its resemblance to a coalsac. . . . There are also several beautiful stars in the Milky which can only be satisfactorily seen through a telescope.

Jupiter has five satellites, with the one latterly discovered, Hyperion. When one of these satellites are exactly between Jupiter and another of its satellites it naturally causes an eclipse, and the satellite farthest from Jupiter will not receive any light from Jupiter (that is to say, the light received from Jupiter from the Sun).

Galileo (whose full name was Galileo Scaparelli) was a Spaniard who did not exactly invent the telescope, but enormously improved on what was then in use, which was an instrument of no more power than our present opera-glasses. By aid of it he made many discoveries about the celestial bodies, which he published in a hidden form. He also discovered the moons of Mars, and two of the moons of Jupiter. The former are called Phroso and Daimio; and Phroso revolves round its primary seven days quicker than Mars rotates.
Galileo was the first to make at all successful observations of the transits of Venus, which were afterwards used for determining the comparative distance of the Sun and the Earth.

In Gemini a star revolves around itself, and both round a third, while all three revolve round one common centre.

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[*] 'Lady Geraldine's Courtship,' by E. B. Browning.

One rather good way of determining the Sun's distance is by parallax. Two observers use an instrument called a micrometer (or else a sextant, when they must also have an artificial horizon), and they measure the angle made by their base-line and the line connecting them with the Sun and measure the base-line, and by a certain theorem in the first book of Euclid they can tell the length of the sides. A still better way is that of using Kepler's third law, as we know the period of E's revolution to be 365·2244 days: we can then square this and, knowing the law, proceed to find the distance.

THERE is a fine idea in a short story which appeared this week. I must confess to a weakness for reading of a frivolous kind when on a railway-journey — it distracts the attention without burdening the memory; and sometimes in the wealth of rubbish one comes across a gem such as appeared this week in a periodical rejoicing in the name of 'Tit-Bits' and sold for a penny—a sort of charlatan's pennyworth, that may contain a prize but usually does not. Well, in the number for April 22 there is an "informer's story" called "A Handshake with Death." The story-teller is supposed to have informed against a gang of anarchists of which he had pretended to be a member. The gang is to be seized by the police at 9 o'clock, the informer among them for the sake of appearances. But he is discovered and denounced at the meeting; the others decamp at 8.30, leaving him helplessly bound, and gazing at a clock which will, at ten minutes to nine, explode a bomb and kill him. The agony of the moments during which he watches the hands of the clock pointing to 8.45, 8.47, and so on, are dwelt upon with blood-curdling effect. At 8.50 we are given to understand that the victim swooned. At 9.0 punctually the police appeared, put the clock and bomb in a bucket of water, cut the prisoner's cords, and roused him from his swoon. Then comes the question, Why had the bomb not gone off at 8.50? The clock had not stopped, and its hands had passed the hour at which the anarchist fiend had set the explosion. The explanation burst on Inspector Grierson, and "an illuminating intelligence shone on his face."

> You say that clock was bought in Edinburgh. Well, you may not know it, but it so happens that Edinburgh time is just thirteen minutes behind the London hour, and to that fact, Mr. Barrington, you owe your life this evening.

Is it not a splendid idea? I do not profess to understand exactly how it works in practice—whether the clock, faithful to its Edinburgh maker, kept Edinburgh time, although its face was showing Greenwich time; or whether the terrified victim read the face thirteen minutes wrong, according to his London instincts; or whether there is some other alternative. I am not equal to solving the question in detail; but it is a magnificent idea—on that point I am clear.

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1899 May 12.

Prof. G. H. Darwin, LL.D., F.R.S., *President*, in the Chair.

Secretaries: H. F. Newall, M.A., and F. W. Dyson, M.A.

The Minutes of the last Meeting were read and confirmed.

Mr. Dyson. 82 presents have been received since the last Meeting, of which the following may be mentioned:—Volume iii. new series, of the 'Annals of the Munich Observatory,' which contains several interesting papers, one on the rotation of Venus and Mercury; volume viii. of the 'Annals of the Bordeaux Observatory'; and we have also received from our President a copy of his new work on the Tides.

A vote of thanks was accorded to the Donors of the presents.

The President. The Council has this afternoon prepared an Address to be presented to Sir George Stokes on the occasion of his forthcoming Jubilee, which I will now read to the Society:—

The Royal Astronomical Society.

To Sir George Gabriel Stokes, Baronet, *Doctor of Laws and of Science, Fellow of the Royal Society.*

We, the President, Council, and Fellows of the Royal Astronomical Society offer you our congratulations in this the fiftieth year of your tenure of the Lucasian Professorship of Mathematics in the University of Cambridge.

We rejoice to pay our homage to one who by his rare intellectual powers has done much to advance human knowledge. By your researches in physical optics you have elucidated the laws which govern the propagation of light and have developed the principles which enable us to study the constitution and movements of the remotest stars. Your investigation concerning the variation of gravity at the Earth's surface will always be remembered as an important contribution to the theory of the figure of the planet on which we live.

Astronomers have thus their special share in the appreciation of your work, and, in common with many other seekers after truth, they owe you a debt of gratitude for the far-reaching influence of the help and sympathy which you have extended to those who have sought your advice and assistance.

Signed and sealed on behalf of the Council and Fellows of the Royal Astronomical Society this 12th day of May, 1899.
G. H. Darwin, *President.*
H. F. Newall, *Secretary.*

The Rev. W. Sidgreaves then read a paper on the "Spectra of γ Cassiopeiæ and o Ceti." He said that the paper, treating, as it did, of the spectra of two stars which had already been extensively studied with the aid of better instruments, could not claim to offer anything new to the Society. It was, on the whole, not more than a confirmation of the observations of others, and notably of those of Sir Norman Lockyer and Prof. Pickering. But he thought that confirmation, when obtained, as in this case, by an instrument of very different structure, was always of real value, and particularly so in the case of the hazy line-spectrum of γ Cassiopeiæ.

The introduction to the paper described the difficulties met with in the reading of the spectrum, and the means employed to avoid misinterpretation. The result of a careful study of over fifty photographs taken during the last eight years was a mixed spectrum of dark and bright lines; and the author had great confidence in the tabulations of both as given in the paper. The table contained, in addition to the hydrogen lines, 44 dark lines and 17 bright lines; with reference to the latter he felt justified in saying that he had no doubt about the reality of these as true gaseous radiations.

The hydrogen spectrum of γ Cassiopeiæ had been given separately, in order to show its peculiarities more clearly. The double character of the lines, as noted by Lockyer and Pickering, was very marked on the Stonyhurst plates, the bright lines resting centrally on broad absorption bands. The intensities of the two series, making due allowance for the sensibility curve of the plate, changed in opposite senses: the bright lines decreased in strength from a maximum in $H\beta$ to zero in $H\epsilon$, the latter appearing only with the strength of the continuous spectrum set off by contrast with the absorption band; the absorption was strongest in $H\epsilon$ and fell off with the slower oscillation frequencies. Bright $H\gamma$ was also seen to be divided, but not with the clearness of separation found on the longer spectra obtained by Lockyer, Newall, and McClean.

The general Orion-type spectrum of γ Cassiopeiæ was exhibited in the wave-length table by comparison with the spectrum of γ Orionis obtained by the same spectrograph, and the helium absorption-lines were collected also in a separate list, together with the lines as determined by Runge and Paschen.

The hydrogen lines had shown no change during the eight years of observation; but in some of the other bright lines there was considerable probability of variations; and of these the one selected for special attention in the future was at D 4586, which on several plates appeared as the strongest after Hγ.

The spectrum of *o* Ceti was photographed successfully on several nights in September and October. The spectrum was found to be in all details the same as at the preceding period of its maximum light. But the Pretender (Hβ as a bright line) was more strongly marked, owing probably to the greater brilliancy of the star.

The paper concluded with a discussion of Dr. Scheiner's hypothesis for the bright hydrogen lines of γ Cassiopeiæ, in reference to the spectrum of *o* Ceti. An extensive hydrogen atmosphere as opposed to intensity of hydrogen radiation was well in keeping with the spectrum of the former, but failed to throw light on the perplexing spectrum of Mira Ceti.

Mr. Newall. Father Sidgreaves has added to our knowledge of two very interesting spectra by his examination of γ Cassiopeiæ and *o* Ceti. Each of these spectra presents peculiar features in the hydrogen lines. In *o* Ceti two of the hydrogen lines are very much brighter than the other ones in the same series, and in γ Cassiopeiæ the hydrogen lines are not only bright, but also each line is double. According to the measurements I have made with a great many photographs of this spectrum, the distance between the two components of these bright hydrogen lines is approximately constant, and there is no sign of orbital motion. One can only infer that the condition of the atmosphere of the star must be somewhat complicated to produce the combination of radiation and absorption observed to exist. Father Sidgreaves's paper is accompanied by a beautifully-drawn map from photographs taken at Stonyhurst College.

Mr. Shackleton showed on the screen photographs of the spectra of the two stars taken at South Kensington.

Rev. W. Sidgreaves (in reply to a question by *Prof. Herschel*). If I have understood your question correctly, it is whether I have taken a positive on glass, and whether I use backing. Positives were taken on glass some years ago. I used backing as a trial for some time, but I gained nothing by it.

Prof. Herschel. I think in such a case the backing would be immaterial, but in copying a print the backing would be most important.

Mr. Newall. I find in my work that backing is unnecessary. The star-spectrum is very much weaker than the comparison-line, and there are no signs of defects in the comparison-lines.

The President. I am sure the Society will not share Father Sidgreaves's very modest estimate of his own work, but will accord him hearty thanks.

Mr. Dyson then read a paper by *Mr. D. A. Pio* on "Longitude from Moon Culminations," which had been sent to the President and was presented by him. It is difficult to say what was new in a paper of this kind on the determination of longitude. The method appears to be able to be described as follows:—Observations of equal altitudes of the Moon are to be taken with a sextant on both sides of the meridian, and observations of the Sun are made to get the local time. The great point is to take the times as accurately as possible. Mr. Pio said that his method was considerably more accurate than the method of lunar distances. It could not be used at sea nor in high latitudes.

The Astronomer Royal. I should like to point out that the principle of this method was used in determining longitude for the Transit of Venus in 1874, but altazimuth instruments were used, which would give much more accurate results. The method of determining longitude by zenith-distance of the Moon with an altazimuth was found to be satisfactory; but I feel a little doubt myself, without having heard the reasons given for adopting this method, as to the advantage of substituting zenith-distance observations with the sextant for the more natural method of measuring the distance of the Moon from stars in the direction of the lunar motion, for which the sextant is peculiarly adapted. I think the subject is an interesting one, and I shall read the paper with interest.

Mr. Knobel. I fancy the times have to be recorded to fractions of a second. In making observations with a sextant it is extremely difficult to record your observations with such accuracy. The nature of the instrument does not permit of it.

Capt. Noble. I do not see why this paper should be said to relate to Moon culminations. I have always understood by Moon-culminating stars, stars which pass the meridian about the same time as the Moon, and that the change in the difference of R.A. of the Moon's limb and of the star caused by difference in longitude is measured by the interval which elapses between their transit over the wires. I used to be very much enamoured of the principle myself and frequently practised it, but with very discordant results.

Prof. Turner. I can assure Mr. Knobel there is no difficulty about observing with the sextant to the tenth of a second if you attach the sextant to a stand. Officers of the R. N. at Greenwich have made such arrangements, and I have taken observations with them. Either by listening to the clock one's self, or by getting some one to record the time, there is no difficulty in making observations to the tenth of a second. But I should like to express a personal opinion that the time for studying any eye-method of determining longitude has gone by. Photography is now, and always will be, very much better. There are two operations in determining longitude. There is the determination of local time, and there is the interchange of signals between the two stations. The value of photography in the determination of local time has yet to be proved; but I have no hesitation in saying that when the telegraph is not available for signals, and observation of the Moon must be substituted, a photographic method is the proper one. Details of such methods have been worked out and practically applied first by Dr. Schlichter, and lately by Capt. Hills, and the accuracy of the observations is many times greater than observations made by the eye with a sextant. The problem to be solved is to find the place of the Moon amongst the stars. We measure in a photograph the distance of the Moon not from one star only but from a number of stars surrounding it, and so obtain much more accurate results than from a single instance.

Mr. Knobel. Prof. Turner has not quite understood what I meant. It was not the sextant as used at a station but as a traveller would use it.

Mr. Dyson. I have been looking through some records of observations of Moon culminations in the Transit of Venus volume this afternoon, and it appears that the error of the determination may be as much as four or five seconds.

Mr. Hollis. I think such errors arise more from the errors of the ephemeris than from the error of the observation. The Moon's tabular place may often be wrong by as much as two-tenths of a second of time, and this is multiplied by about thirty, so that large errors are easily accounted for.

The President. The discussion which has arisen on this subject is sufficient proof of the interest in Prof. Pio's paper, and we must give our thanks to him for his communication. I will write to him and say it is recommended for printing. It is flattering to us that a Greek should choose the Royal Astronomical Society as the medium for giving his paper to the world, and that he should write it in English.

[*The Astronomer Royal* now took the Chair.]

The Astronomer Royal then read a paper on observations of the Satellite of Neptune from photographs taken with the 26-inch refractor of the Thompson equatorial. Photographs of this satellite had been taken regularly at the Royal Observatory with the Thompson equatorial both with the reflector and the refractor. In the latter period the refractor was utilized, and an occulting shutter was fitted up so that a series of short exposures could be given to Neptune, and thus accurate measures could be made with the small image so obtained during the long exposure on the satellite. This paper embodied the results of those measures. The point was that they had given twenty exposures of one second each to Neptune, and an exposure of twenty minutes to the satellite, and had obtained measurable images of both, and the results of the measures were given in the paper. The main interest in these photographs was that they gave a case in which photography was eminently applicable to precise measurement, and that they could really get much more accurate measures with less expenditure of time by photography than by visual observations. Four independent sets of measures of each photograph had been made by two observers, the whole time required for measurement and reduction being only half an hour. A comparison of the results showed that very great accuracy was obtained. The mean discordance of the four sets of measures was $0^{\circ}\cdot30$ in position-angle and $0''\cdot13$ in distance, which was a very satisfactory result. Twelve photographs had been obtained with the occulting shutter which were capable of accurate measurement. The photographs of Neptune's satellite could not be commenced as soon as could have been wished owing to the attention which had to be given to photographing the planet Eros, which was considered to be more important.

Prof. Turner. I should like to ask about the measures. Are they available for determining at the same time the position of the stars?

The Astronomer Royal. We have not discussed them with reference to that, but simply to obtain position-angle and distance of the satellite from Neptune. There was no idea of determining the position of Neptune, which would have been much more laborious.

The President. May I ask for how many years past has it been possible to photograph such faint objects as satellites?

The Astronomer Royal. I can scarcely say exactly, but I believe Dr. Roberts has photographed the satellite of Neptune.

The President. And Uranus?

The Astronomer Royal. I am not sure about Uranus, because it has been very unfavourably placed. It is not so much the question of photographing the satellite as of getting measurable photographs. That is the problem we have been attacking—to get concurrently with a distinct image of the satellite a small image of Neptune with short exposure, so that the position-angle can be measured accurately.

Prof. Turner. There is this difficulty in the case of Eros—the stars around it are changing rapidly, but Neptune is a planet which remains conveniently in the same position, which makes the reduction of several plates less laborious than it would otherwise be.

Prof. Turner then read a second paper on the subject of optical distortion. At the last meeting he had shown reason for supposing the optical distortion over a region four times as large as the Oxford astrographic plates, photographed at Arequipa with the Bruce doublet, to be nearly insensible. Some outstanding errors were traced with great probability to errors of copying the Arequipa positive. In the present paper this supposition of errors of copying was confirmed by direct experiment. Secondly, a portion of the Arequipa plate still further from the centre had been examined and no sensible optical distortion found. Finally, the linear terms in the comparison of the two plates, hitherto not considered, had been examined and no evidence of distortion found. The whole investigation had been laborious, and he had now found that it might have been conducted on simpler lines, though he was glad to have gone through with it from this point of view

The simpler method was that suggested by Capt. Hills at the last meeting of taking star-trails in different parts of the plate and measuring the curvature. He had been looking at the necessary reductions, especially that for differential refraction, and had found them very simple indeed. This was no doubt the best method of procedure.

Mr. Dyson. It is most valuable to know that in a plate six degrees square there is no distortion. One can hardly speak about the method of trails because Prof. Turner has not communicated that paper to-night. The objection I should make to Prof. Turner's method is that when the distortion is small the very small quantities involved may be rather swamped by such things as the effect of inclination or curvature of the plates in the copying. It would seem desirable to take the plates themselves, if possible, and measure them without any chance of introducing further errors of that kind. Prof. Turner, at any rate, has got to this extent that the errors are exceedingly small, if they are not entirely done away with.

The Chairman. The question is one of extreme interest in the present stage of the application of photography to accurate astronomical measurements. It lies at the root of the whole question, and we will give our thanks to Prof. Turner for having contributed much which throws light on this subject.

The Chairman. We have present this evening Mr. Warner, who will be known to you all as a member of the distinguished firm of Warner & Swasey, who have done so much on the other side of the Atlantic. We welcome him here, and shall be glad to hear any account that he can give us of what is being done in America.

Mr. Warner. It is a great pleasure to meet you. This is the first time I have had the pleasure of meeting you since I have been able to call myself a Fellow of this Society, and I begin by thanking you for electing me to that position. I esteem it a great pleasure and an honour, and I hope you may not regret it. With regard to our work on the other side of the water, the large telescope which we have been trying to make has been completed, and has been doing work for some time. The most important new thing that is now on hand is being developed by a man who I know is a friend of yours, Mr. John A. Brashear, of Allegheny. All those who know him love him. He has been raising money for building a large Observatory at Allegheny, to be the successor of one which was used by Prof. Langley, who is now Secretary of the Smithsonian Institution at Washington. Mr. Brashear has raised £40,000 for this Observatory, and the telescope is to be 30 inches aperture, and to be equipped in the most modern style. This money has been given by people in the neighbourhood who know him so well that they have trusted the money to him. It is believed that in two or three years it will be completed. Astronomers on the other side are devoting themselves to specialities. You are familiar with the work of Pickering, Hale, Burnham, and others. But besides such observations as these we have been making others.

We observed the movements of your English astronomers a year or so ago when you came to America, and we have kept track of you and have computed your orbits and decided very accurately when you ought to return. It is believed that, to be up to time, the Astronomer Royal and Prof. Turner, and the others who came before, are due again next year, and if for any reason they fail to make their appearance we are going to charge it to some error in personal equation. (Laughter.) I want to assure you all that there is no place where you will be more welcome on this globe than among the Yankees of the United States. (Applause.)

The Chairman. I need not ask you to return your thanks to Mr. Warner because you have already done so.

Mr. Newall then read a paper by *Dr. Hartwig* on Prof. Schur's paper on the Polar Compression of Mars.

The following papers were announced and partly read:—

L. A. Eddie. "Observations of Swift's Comet 1899, made at Grahamstown, South Africa."

Joseph Gledhill. "Observations of Mars made at Mr. Crossley's Observatory, Bermerside, Halifax, 1898–99."

Rev. W. Sidgreaves. "Note on the Spectra of γ Cassiopeiæ and o Ceti."

D. A. Pio. "Longitude from Moon Culminations."

Royal Observatory, Greenwich. "Observations of the Satellite of Neptune from Photographs taken with the 26-inch Refractor of the Thompson Equatorial."

E. Hartwig. "Remarks on the paper by Prof. W. Schur, together with Determination of the Diameter and Polar Compression of Mars from Observations at the Bamberg and Strassburg Observatories."

H. H. Turner. "On the Errors of Star Photographs due to Optical Distortion of the Object-glass with which the Photograph is taken." Second Paper.

E. M. Antoniadi. "Note on the Constitution of Saturn's Crape Ring."

The following gentlemen were elected Fellows of the Society:—

Rev. E. L. Berthon, M.A., St. Margaret's, Cupernham, Romsey, Hants.

Rev. T. E. R. Phillips, M.A., Hendford Vicarage, Yeovil, Somerset.

The following Candidates were proposed for election as Fellows of the Society:—

Rev. T. G. Barber, M.A., 10 Highfield Road, Doncaster (proposed by J. W. L. Glaisher).

S. S. Hough, M.A., Royal Observatory, Cape of Good Hope (proposed by D. Gill).

The following foreign astronomers were proposed by the Council as Associates:—

G. E. Hale, D.Sc., F.R.A.S., Director of the Yerkes Observatory, Williams Bay, Wisconsin, U.S.A.

F. R. Helmert, Director of the Geodetic Institute, Potsdam, Germany.

F. Küstner, Director of the Observatory, Bonn, Germany.

Juan M. Thome, Director of the Argentine National Observatory, Cordoba, Argentine Republic.

THE BRITISH ASTRONOMICAL ASSOCIATION.

The Annual Conversazione of the British Astronomical Association was held at Sion College on Wednesday evening, April 19. The President, *Mr. W. H. Maw*, received the Members and Visitors from 7.30 to 8 o'clock. During the evening three addresses were given, illustrated in each case by the lantern. *Mr. A. C. D. Crommelin* took for his subject "The Photography of Comets." He pointed out, in the first place, the great value of photography in the discovery of comets. A comet was detected on the photographic plate by its trail, and the direction of the trail showed the direction of the comet's motion. Photographs also showed an immense amount of detail in the structure of comets that had not been previously suspected, and which could not be observed by the naked eye. Photography also gave a better determination of a comet's place than an ordinary eye-observation.

Capt. Molesworth, R.E., read a paper on "Observations of Jupiter's Satellites." Referring to the familiar fact that the first satellite appeared to change its shape when in transit across Jupiter, he explained that this was due to the fact that it has dark poles and a brilliant equator, and, as seen in certain places only the bright equatorial belt was visible, so that the satellite then seemed to be elliptical in shape. This was admirably illustrated by a specially prepared set of lantern-slides. With regard to the third satellite, he had been able to see such a large amount of detail on its surface as to enable him to determine its rotation-period and make a chart of its surface. Capt. Molesworth's observations had been made in India, and he referred to the excellent atmospheric conditions he had usually experienced.

Mr. Walter Goodacre gave an address on "The Moon as illustrated by Photography." He showed on the screen lantern-slides of the Lick and Paris lunar photographs, besides a number of reproductions of drawings of lunar details by Members of the Section.

A large number of astronomical photographs and drawings were on view during the evening, including the Astronomer Royal's photographs of the eclipse of January 1898. Most of the principal astronomical instrument makers exhibited telescopes, and Mr. Thorpe, of the Manchester Branch, had a most interesting exhibit of spectroscopes and gratings of his own designing.

During the evening selections of music were given by the Blue Anglo-Hungarian Band.

ROYAL METEOROLOGICAL SOCIETY.

Owing to the public improvements in the neighbourhood of Parliament Street, the Royal Meteorological Society has been obliged to vacate its offices in Great George Street and find accommodation elsewhere. The Council ultimately took rooms at Princes Mansions, 10 Victoria Street, which have been fitted up to meet the requirements of the Society. On Tuesday evening, the 16th May, the President, *Mr. F. C. Bayard, LL.M.*, held an "At Home" in these new rooms, which was largely attended by the Fellows, among whom were the following:—Mr. W. B. Bryan, Mr. Baldwin Latham, Prof. J. K. Laughton, Admiral Maclear, Dr. H. R. Mill, Dr. R. H. Scott, F.R.S., Mr. G. J. Symons, F.R.S., Dr. C. Theodore Williams, Mr. E. Woods, and others.

An interesting exhibition of instruments, photographs, &c. was arranged in the various rooms, and there were also several demonstrations by the optical lantern.

At the Monthly Meeting of the Society, held on Wednesday afternoon in the above rooms, *Mr. H. H. Dickson, F.R.S.E.*, read a paper entitled "The Mean Temperature of the Surface-waters of the Sea round the British Isles, and its relation to that of the Air."

A paper by *Major-General Schaw, C.B.*, on some Phenomena connected with the Vertical Circulation of the Atmosphere," was also read.

Notes on Mars in 1899.

My observations of Mars only began this year on February 23, that is, more than a month after Opposition, the apparent diameter of the planet being then reduced to 12″. Hence there is nothing surprising in the fact of there not being anything very startling to announce, especially since my eye invariably requires at least two months' continuous observation of a planet before it acquires its full sensitiveness to the most delicate details. In the case of Jupiter this kind of training has to be gone through every year. Notwithstanding these unfavourable conditions, a few facts were noted which may not, perhaps, be altogether without interest.

The observations were made with a power of 320 on a 6½-inch reflector by Calver. A power of 450 was also occasionally employed with advantage whenever the seeing was good enough.

Altogether 28 of the narrow dark streaks, commonly termed "canals," were seen in the course of the observations. At first, owing to the lack of training alluded to above, most of these appeared only under the form of more or less diffuse streaks. Some observers seem to see *all* the canals as sharp, narrow, definite lines. This is contrary to my experience, according to which some of them invariably appear broad, diffuse, and indefinite, however perfect may be the definition, and however sharply defined neighbouring canals may appear. Thus, I have never seen the Ganges, whether single or double, other than broad and diffuse. But when the eye is not in perfect training nearly all the canals have this aspect of broad diffuse streaks. Towards the end of the observations, however, when my eye had received a certain amount of practice, the canals appeared much more definite.

Two or three canals were seen more or less distinctly double. Nilokeras, for instance, was almost certainly *anomalously* double (*i. e.* with the component lines not parallel). Jamuna also appeared to be double. In neither of these cases, however, was the duplicity absolutely certain, owing to indifferent seeing. But the Gehon was seen conspicuously and undoubtedly double on March 2. Two distinct streaks ran parallel to one another in a straight line from either inlet of Dawes's Forked Bay to the Mare Acidalium. The canal was also seen double on March 1. Schiaparelli's early maps of Mars show the Gehon running in a curve to the Mare Acidalium, but it is noteworthy that on the present occasion both components of the double canal seemed to run in a perfectly straight line from Dawes's Forked Bay to the Mare Acidalium. Considered as a single object the double Gehon was very conspicuous.

The Cerberus was seen even more distinctly double than the Gehon upon one night, March 15. Definition was pretty good at times, and the canal appeared most distinctly double with 450. Owing to the small size of the image (apparent diameter of planet = 10″), the duplicity was only suspected with 320, but with the higher power it was undoubted. The canal, in fact, was seen double with as much certainty and distinctness as a double star near the limit of separation for the aperture employed.

Perhaps it may be well to give here my definition of a double canal. According to my view a double canal is merely due to two ordinary single canals being seen apparently close together. If both single canals are narrow and definite, and run nearly parallel to each other, the resulting gemination will be perfect, the included region appearing as bright as the exterior. But if one or both of the single canals should be diffuse and irregular on the edges, then the included area will appear more or less dark or shaded. This is a very common form. Few things are better established about

Mars than the fact that individual single canals vary largely in plainness from time to time, and sometimes apparently in position, and a very little variation of this kind in one or both of the adjacent single canals would suffice to cause the duplicity to be apparent or invisible. Further, when a double canal is observed with high powers under nearly perfect conditions of seeing, the parallelism of the component lines is seldom quite perfect. In other words, most double canals are really anomalously double. It may be stated here that actually the margins of most canals are obviously a little irregular, and that such irregularities are not infrequently of considerable magnitude. Lastly, I should like to express the belief that if we could approach Mars to within a few miles, the appearance presented by these so-called "canals" would be so changed that we should not recognize them at all.

A few of the single canals presented peculiar or abnormal aspects during the recent observations. The Nilosyrtis, the largest and most prominent canal of Mars, was remarkable for its faintness in March. It appeared but little plainer than the Amenthes, which was quite faint, and was very irregular both in width and darkness, and perhaps actually interrupted in places. The Eunostos, on the other hand, was broad, and extremely dark and conspicuous on its north following half, so as to resemble an immense gulf or inlet extending in a south-east direction from the black border surrounding the polar snow-cap into the bright continental regions.

Lacus Moeris appeared as a round, very dark, in fact almost black-looking spot, open on the following side to the Syrtis Major, though once or twice a narrow bright border separating it from the latter was suspected. But although the lake was so prominent, the canal Nepenthes in connection with it was not seen.

The dark spot called Trivium Charontis is a more than usually interesting object. In 1894 it appeared to me of a triangular shape with light interior *. This year it has again appeared under the same form. The triangular shape was more or less obviously seen on March 14, 15, and 17, and the object had the appearance of being formed by three or more canals crossing one another, instead of meeting at a point, the result being an open triangle, with the apex directed to a point between the canals Orcus and Tartarus, and the base being formed by the Cerberus, at this point single. A drawing made by Mr. A. E. Douglass † on January 18, 1899, shows the triangular form of the Trivium Charontis very distinctly. This and other drawings by the same observer also show the canal Cerberus *anomalously* double. Whenever one canal meets or crosses another it is common to find a little dark spot, or lake, at the point of meeting or intersection. An intensification of two such spots at the north and south angles of the triangle might cause the Trivium Charontis to appear under the form of

* Memoirs B. A. A. vol. iv. plate ii. fig. 10.

† 'Popular Astronomy,' March 1899.

two round black spots, as observed by M. E. M. Antoniadi at Juvisy * in 1896.

The northern snow-cap was always very brilliant, and usually appeared bordered on the south side by a black streak. On one night (March 15) a detached bright spot was noted south of this black border, in about longitude 210°. This detached spot had not, however, the glittering brightness of the true snow-cap, and may have been only a cloud-spot.

A. Stanley Williams.

Note on the Line of Collimation of a Transit-Instrument.

Some remarks in the March number † on the collimation of a certain transit-instrument having elicited some expression of interest, it is proposed to follow these by making some general remarks on the geometry of this subject. The problem to be considered may be stated thus:—In the text-books the line of collimation of a transit-telescope is defined as a line which is at *right angles* to the axis about which the telescope turns, and therefore describes a great circle in the sky as the instrument is rotated. Also from the definition it follows that if the instrument be reversed on its pivots so that the pivot which was West is now East, the great circle described is identical with that described before reversal, assuming that no change of position of the bearings has taken place either in azimuth or level. This identity of the circle described before and after reversal is tested by the error of the clock as shown by transits of stars, including close polar stars, but it is not always that accurate identity is established. In so many accounts of longitude determinations there are found discordances of clock-error as found in the two positions of the instrument, instances of "polar" collimation differing from horizontal collimation, of determinations of the line of collimation which differ in the two positions of the instrument, that there is reasonable ground for thinking that it is impossible to know how much the supposed line of collimation of a telescope may be in error unless it be reversed in its bearings.

The methods of finding the position of this line in a telescope may be classified under two heads:—(1) Reversal on a distant mark; the mark being either the nadir-point, as given by mercury in a basin vertically below the axis of the instrument, an actual mark in the plane of the horizon, or a star near the pole. (2) By a pair of horizontal collimators, in which case the line is found without reversing the instrument. There is a distinct difference of principle in the two cases. In (1) a line is found which points to the *same point* in the two positions, and should be therefore at right angles to some line or plane of the telescope which has the same direction, but with opposite sense, in the two positions of the instrument; in (2) a line is found which is in the same direction when the telescope is rotated through 180°, and should therefore be at right angles to a line which does not change its position as the instrument rotates. This is elementary but the point of this difference will be seen later.

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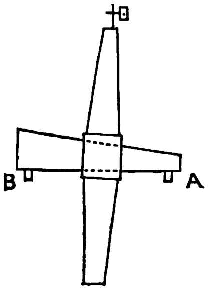
* Bulletin Soc. Ast. de France, 1897, p. 41.

† The Leyden—Greenwich Longitude, p. 117.

The first reason which suggests itself for discordances such as above mentioned is a pivot-error; the diameters of the two pivots may not be exactly equal. It may be expedient to briefly consider here the result of such an inequality. If one pivot be larger than the other, the instrument moves as though its axis were a cone the radius of whose base is equal to the difference of radii of the pivots. This is represented with exaggeration in fig. 1. By reversal on the nadir, a line is found at right angles to the slant side, AB, of the cone which lies in the same line in both positions. By reversal on a horizontal mark the line is found at right angles to the axis of the cone, or rather to the vertical plane through the axis, which is the same in both positions. Hence the difference between the determinations of collimation from the nadir and from a horizontal mark is an exact measure of the inequality of pivots. It is not necessary to pursue this further, suffice it to say that the collimation determined by reversal on a polar star would differ from either of those just mentioned; and in fact if the collimation line be determined from nadir observations an inequality of pivots gives rise to a collimation-error and a level-error of opposite sign in the two positions of the instrument; but there are independent methods of finding this inequality, so that it may be assumed the proper corrections have been made, and that we have an instrument turning about the axis of the hypothetical cone above illustrated, which axis is accurately level and lies truly in an east and west line.

Fig. 1.



A reason for this discordance that we propose now to consider may be called Flexure. As to what this flexure actually is, whether a bending of the axis or of the tube, an unequal contraction of the tube, or non-symmetry of counterpoises, no assumption is made, but we recall the paper by Sir George Airy (Mon. Not. R. A. S. vol. xxv. no. 3) in which he shows, from consideration of the effect of gravity on different particles of the instrument, and the difference of direction of gravity relative to axes of bending, that there is a line of the telescope which describes a great circle, but that the plane of this circle is not at right angles to the axis of rotation. This we believe has been generally accepted, and there seems to be reason to take as definition, "the line of collimation is the line which describes a great circle," without making assumption as to its inclination to the axis of rotation. It appears to be a result of Airy's investigation that when the instrument was reversed on its pivots, not only that this line still described a great circle, but also that it described the *same great circle in the sky* *. It is difficult to see this in some ways. It seems, for instance, to lead to the following paradox.

Fig. 2.

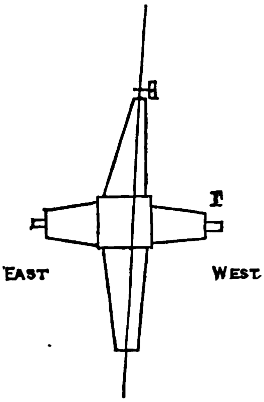
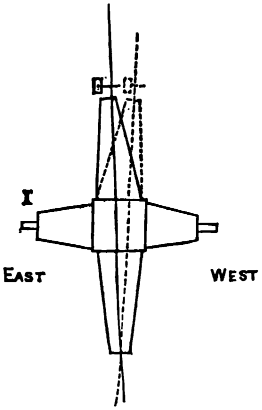


Fig. 3.



The diagram (fig. 2) shows the telescope pointed to the nadir, with the pivot (I) on the west bearing. The long black line shows the line of collimation as above defined, the object-glass end of which *now* makes an obtuse angle with the axis towards (I). In Airy's paper it appears that after reversal, as in fig. 3, this same line, which has the same micrometer-reading as before, would be represented by the dotted line, which towards the object-glass end makes an acute angle with the end of the axis (I). This, as we have said, is hard to see, because the physical circumstances all remain the same, except possible differences of the bearings; but this point is not being considered. Moreover this does not help to explain facts of observation. We propose to consider some results from the hypothesis that the same line describes a great circle in both positions of the instrument; not the same circle in the sky, but circles symmetrical with respect to the plane at right angles to the axis of rotation, and intersecting that plane in the *same line* in both positions.

The first thing to be considered is the position of the line where the meridian of the instrument, as this great circle may be called, intersects the plane at right angles to the axis, *i. e.* the true meridian if the axis be truly placed, as we have assumed. If the line be in the horizon, the error caused by flexure is simply a level-error. This level, it will be seen, can be at once determined by a mercury basin if the micrometer-reading of the line is known: but the level, as determined by striding-level, will differ from this—in fact the circumstances are almost exactly analogous to those which result from inequality of pivots. The true collimation-reading can be obtained by reversal, but only on a horizontal mark. It can be found by opposing collimators in whatever line the two circles intersect; and here we are bound to admit that our hypothesis fails to explain cases where a different collimation has been found in the two positions of the instrument by horizontal collimators*; but it does supply a geometrical reason for the finding of a different micrometer-reading from nadir observations and from observations of polar stars. Referring to figs. 2 and 3 above, assume that the micrometer-reading increases as the wire is moved towards the micrometer head, that the false meridian intersects the true on the horizon, and that the reading of the line of collimation (which is the true one) has been determined by a horizontal mark. If the telescope be turned downwards, in both positions, fig. 2 and fig. 3 the reading will have to be diminished to point the line to the nadir, and this smaller reading would be the apparent line of collimation. Now turn the telescope to the zenith; the black line still represents the true line of collimation and remains in the same direction, and to make it pass through the zenith the micrometer-reading must be increased in both positions of the instrument, so that the micrometer-reading for collimation from a zenithal star would be larger than the true. This reasoning may be applied to any general case: the collimation obtained from reversal on a mark on the line of intersection of the true and fictitious meridians, wherever it may be, is the true collimation; the apparent collimation found by reversal on a point in any other direction is in error by a quantity which changes sign as it passes through the above defined direction, and is a maximum at 90° from it.

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* This is not explicitly stated, but it is inferred from the formulæ and diagrams.

It is easy to examine other particular cases. Suppose the line of intersection of the meridian of the instrument with the true meridian passes through the zenith. This is the case when the flexure is zero when the telescope is vertical. The effect of the hypothesis now suggested is to give an error solely of azimuth, the same but with opposite sign in the two positions of the instrument. The collimation determined from reversal on the nadir would be the true line (it would not agree with that determined from a mark on the horizon or from polar stars), and the same observations would give the true level. Also the azimuth-error determined in *either* position of the instrument would give the appropriate correction for the observations made in that position. This would be an easy case to deal with in practice; but it must be said that it is not easy to give a simple physical reason to explain the circumstances, but at this moment we do not feel called upon to do this. The hypothesis is put forward to explain some facts, with the hope of continuing the subject on a future occasion.

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* See "India's Contribution to Geodesy," Phil. Trans. vol. 186, p. 795.

H. P. Hollis.

Prof. Barnard on the Gegenschein *.

In 1883 so little was known or written about this object that I had never heard of its existence. One night in the beginning of October in that year, while comet-seeking at the Vanderbilt University, Nashville, Tennessee, my attention was attracted by what appeared to be a spot of local haze south-east of the square of Pegasus. Noticing this same appearance the next night, I watched it, and soon found that it was not a spot of haze, but that it remained constant all night, and a few nights' observations convinced me that it was a real object belonging to the sky, and that it was moving eastward about one degree a day. Since then I have paid much attention to this object, and have found out several things previously unknown about it.

My observations seem to negative any inclination of the path of the gegenschein to the ecliptic. It doubtless lies exactly in the ecliptic and always exactly opposite the Sun. This of itself is suggestive of its being a phenomenon due in some way to the Earth alone. The fact that the gegenschein is seen perhaps better on a not too pure sky is also suggestive of its non-existence as a celestial body in the ordinary sense of the word. To me it has always appeared to be an atmospheric phenomenon; but there are several objections to this explanation, the main one being the absence of any sensible parallax. If it were refracted sunlight reflected from opaque particles in the atmosphere there would of course be an enormous parallax. This explanation therefore seems improbable, yet I cannot feel satisfied that the gegenschein is not in some way connected with our own atmosphere.

No satisfactory theory has yet been offered to explain the gegenschein, though one connecting it with the asteroidal zone has received some consideration and apparently has much in its favour, but there are objections to the theory which cannot easily be reconciled with the observed facts.

For the popular exposition of this theory I will quote from a previous article of mine on the subject:—

"Between the orbits of Mars and Jupiter are known to be no less than four hundred small planetary bodies. It is probable there are thousands and possibly millions more of these dust worlds. The smaller ones found in recent years are perhaps not over 10 or 15 miles in diameter. As they decrease in size doubtless they multiply in number until finally they exist in untold multitudes of a size comparable with small stones and particles of dust, which no telescope will show individually. Each one of these small bodies is a miniature planet, and must shine as a planet by reflecting the light of the Sun. It therefore must present to us phases just as Mars does, but from the smallness of the objects we cannot see these phases—but none the less they must have an effect on the brightness of each little asteroid, and must diminish its light accordingly.

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* Extracted from an article by Prof. E. E. Barnard on this subject in 'Popular Astronomy' for April 1899.

"Let us assume there are a sufficient number of these tiny planets. Though they may be too small to be seen individually in a telescope, their combined light may be so great as to affect the eye, and thus we might expect to see a feeble zone of light extending across the sky and corresponding to the asteroidal zone. Such we actually have in the zodiacal band. What would be the effect of phase of these individually small bodies upon the appearance of this zone of light? When opposite the Sun, each of the objects would shine with full enlightened disc, and this vast collection of fully illuminated particles would augment the light of the zone and give a greater luminosity immediately opposite the Sun. Away from opposition and the phase would begin to diminish the quantity of illumination. To this must also be added the fact that at opposition each asteroid would be nearer us and brighter from this cause also. Both causes would tend to give a maximum of light opposite the Sun—a gegenschein."

This theory, I believe, is due to Professor Arthur Searle of Harvard College Observatory.

In the 'Observatory' for January, 1899, Mr. J. Evershed offers an explanation of the gegenschein, which assumes it to be a tail to the Earth, produced by the escape of molecules of hydrogen and helium away from the Earth in a direction opposite the Sun in a similar manner to the formation of a comet's tail. The absence of any parallax is fatal to this theory.

I have often noticed late in the afternoon, when my back was towards the Sun, a brightish glow on the dry grass about the shadow of my head, due to the rapid increase of shadow effect away from the point directly opposite the Sun. Near the shadow of the head, the shadows of the grass, &c., would be hidden by the grass itself, and that region would appear brighter than at a slightly greater distance, where the shadows would become exposed, and hence produce a lessening of brightness away from the shadow of the head; the bright spot about the head is therefore a gegenschein, and this ought to be suggestive of an explanation of the real gegenschein.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Early Lyrids and Early Comets.

GENTLEMEN,—

It is possible to find mention of other meteoric showers that were, probably, ancient appearances of the Lyrid group, in addition to the "ten probable ones" collected by Professor Newton. For instance, we are told, in A.D. 764, "The March after was a continual fall of stars" (Chron. Franç.). In A.D. 829, after the mention of an earthquake at Aix a few days before Easter, we are told "very many little twinkling fires, like stars, ran up and down the air" (Chr. Magdeb.). In 1389 April, "fiery dragons." In 1396 April, "many dragons." This last mention may be a little bit uncertain, as it is elsewhere recorded that in April of that year "was seen a fiery dragon (comet) in many parts of England which surprised many." These extracts are from a 'General Chronological History of the Air, Weather, Seasons, Meteors, &c.,' London, 1749.

The same work records a number of comets seen in Great Britain before the one commonly supposed to be the first, that of A.D. 497, but the chroniclers of these are not stated. "Comets:— A.D. 42. 'Over London.' 50. 'Dreadful over London.' 57. 'England; great famine and dearth after.' 90. 'Seen at Winchester.' 109. 'England.' 127. 'Scotland, the plague after.' 137. 'Over York; the plague soon after.' 169. 'Wales; plague quickly after.' 211. 'Five weeks over London; a sore plague after.' 217. 'Over Edinburgh.' 235. 'Great war and bloodshed after, in Wales.' 249. 'Britain; great war after.' 280. 'Great war after it; Scotland.' 295. 'Wales; grevious famine after.' 321. 'Britain; great bloodshed after.' 329. 'For four months over Britain.' 418. '...and plague, famine, inundations, meteors; it shined to September.'" (This is the comet first noticed during the eclipse of the Sun at Constantinople, and seen for a long time afterwards, and passed over the last star in the Bear's tail.) "452. 'Great devastation after, by fire and sword, in Scotland.' 480. 'In Scotland; great plague and famine after.'" A few of these were observed in China, but doubtless some could be disposed of by the supposition of an auroral appearance.

Melplash Vicarage, Bridport,
1899, April 22.

Faithfully yours,
S. J. JOHNSON.

Three Lunar Eclipses in a Year.

GENTLEMEN,—

The rare coincidence of three eclipses of the Moon being visible at Greenwich between January 1 and December 31 last year is an inducement to search for previous years when this took place. It seems to have occurred sixteen times since the commencement of the Christian era, viz., in the years A.D. 247, 307 (one eclipse beginning very shortly before sunrise), 391 (one eclipse very small), 763, 809, 893, 1023, 1154, 1330, 1414, 1479, 1544, 1703, 1768, 1833, 1898. This does not seem to happen again until the year 2178; but it would be the case in Ireland in 1917, owing to difference of longitude. In two years above mentioned, 893 and 1544, some part of the "total" phase of three eclipses of the year would be seen.

Faithfully yours,
S. J. JOHNSON.

Melplash Vicarage, Bridport.
1899, April 22.

Conon and Coma Berenices.

GENTLEMEN,—

Although the astronomical observations of Conon of Samos, the friend of Archimedes, are referred to by Ptolemy, little is known of them, and he is chiefly remembered by the fact of his having given a name to a small group of stars near the tail of Leo (of which it might well form a part), between that and the southern part of Boötes. This name is first found as that of a distinct constellation in the catalogue contained in the Rudolphine Tables formed by Kepler (with some additions) for the epoch 1600, from the observations of Tycho Brahé. In that incorporated in the 'Historia Cœlestis' (epoch 1593) it appears, but only as a portion of "Complementum Leonis." The arrangement by which Coma Berenices was accepted as a separate constellation was adopted by Flamsteed, and followed generally in subsequent star-catalogues.

The story of how Queen Berenice, of Egypt, vowed to dedicate her locks of hair in the temple of Venus if her husband should return in safety from his Syrian expedition, how they disappeared in some mysterious way from there, and how the courtly astronomer maintained that he saw them exalted into the sky, is well known. But there has been some confusion as to which of the Ptolemies was her husband, Lalande stating that it was Soter, the son of Lagus (and founder of the dynasty), though Conon was mathematician to his son Ptolemy Philadelphus. The Berenice in question, however, was the wife of Ptolemy Euergetes, the son and successor of Philadelphus. We are indebted for our knowledge of the story to a poem of Callimachus, and to a translation thereof (as the original is not extant) by Catullus, of whose odes it forms the sixty-sixth.

Speaking in the person of the 'Coma,' he writes (v. 7–9):—

> Idem me Conon cœlesti lumine vidit
> E Bereniceo vertice cæsariem
> Fulgentem clare;

and later (v. 63–68):—

> Uvidulam a fletu, cedentem ad templa Deum, me
> Sidus in antiquis Diva novum posuit:
> Virginis et sævi contingens namque Leonis
> Lumina, Callisto juncta Lycaoniæ,
> Vertor in occasum, tardum dux ante Booten,
> Qui vix sero alto mergitur Oceano.

Conon is said to have made observations in Italy; but, however that may be, his reputation was widely diffused, and his name is mentioned in the third eclogue of Vergil, together with another who is left anonymous and may possibly be Aratus:—

> Conon et qui fuit alter,
> Descripsit radio totum qui gentibus orbem,
> Tempora quæ messor, quæ curvus arator haberet,

the two being stated to be depicted on the two bowls which were staked by Menalcas in a pastoral contest on pipes, which ended in a tie.

Yours faithfully,

Blackheath, 1899, May 8. W. T. Lynn.

The Causes of the Gegenschein.

Gentlemen,—

Mr. Backhouse is mistaken in supposing (page 162, April number) that I have suggested that the Gegenschein and Zodiacal band are distinct objects, and the latter half of his first sentence exactly expresses the view which I advanced in the 'Observatory' of November last, page 408, although he appears to think to the contrary. When I stated (page 94, February) that I believed that more than one phenomenon had been called the Gegenschein, I referred to the fact that some of the recorded observations appear to be applicable to the opposition portion of the band itself, while others are certainly not so applicable (see the 'Observatory,' August 1897, page 327).

The chief difficulty which I have had in accepting Mr. Evershed's hypothesis lies in the fact that the Gegenschein appears to be obviously connected with the Zodiacal band, which itself is but a continuation of the Zodiacal Light. His hypothesis appears to necessitate the admission that the Gegenschein is a perfectly distinct phenomenon superposed upon the Zodiacal band.

As seen from Madeira during the past two years, the band extending from the apex of the western cone of the Light right across the sky is brightest and broadest from about November until February, so that unless this is a purely seasonal or atmospheric effect, the Earth is either much more deeply immersed in the disc of particles composing the Zodiacal Light at this time, or else the disc is much more dense at this portion than elsewhere. Probably the fact that the Earth is nearest the Sun during this period is the cause of the increased brightness of the band, the outer limit of the disc being then further beyond the Earth's orbit than in summer, as I indicated in fig. 2 on page 376 of the 'Monthly Notices' for May 1898. If this be the case, and observations from the southern hemisphere can alone decide the point, then the disc of particles can scarcely extend to tens or hundreds of millions of miles beyond the Earth's orbit, as Mr. Backhouse thinks probable (page 162, April number), for in that case the three millions of miles which the Earth is nearer the Sun at perihelion than at aphelion could make little or no perceptible difference in the brightness of the band.

Possibly atmospheric absorption may account for some of the discrepancies in the variously estimated positions of the centre of the Gegenschein. Observations made before midnight might be expected to give a centre somewhat west of the true position, and *vice versa.*

The promised publication of all his observations of the Gegenschein down to 1895 by Mr. Backhouse will be a very valuable contribution to the available literature on the subject.

Yours very sincerely,

Madeira, 1899, May 1. Wm. Anderson.

The Queen's Birthday and the Foundation of the Astronomical Society.

Gentlemen,—

Whilst we are rejoicing in the completion of Her Majesty's eightieth year, it will be to us not without interest to recall that the actual day of her birth coincided very nearly with the first effective suggestion of the foundation of a Society for the promotion of astronomy, which was so soon to enter upon its course, notwithstanding the unfortunate opposition of Sir Joseph Banks, from a mistaken notion that its work would clash detrimentally with that of the Royal Society, of which he was then President. It was on 1819, May 20, that Francis Baily circulated his translation of Cagnoli's memoir on a method of ascertaining the true figure of the Earth by occultations of fixed stars as observed in different latitudes, in the appendix to which he dwells upon the benefit that would accrue from the means of co-operation on a large scale in astronomical observations and other advantages that might be expected to result from associated efforts in the advancement of the science. In other arts and sciences, he remarks, "institutions have been formed for the purpose of promoting and diffusing a general knowledge of those particular subjects. . . . . . But Astronomy, the most interesting and sublime of all the sciences (and, to our country, certainly the most useful), cannot claim the fostering aid of any Society. It is, however, well known that many individuals, in various parts of the kingdom, have of late years erected and furnished private observatories at a very considerable expense. Nevertheless the utility of those establishments must be greatly circumscribed through the want of some mode of general communication amongst observers, by means of which their labours might be collected and registered; and thus rendered permanently useful. The formation of an ASTRONOMICAL SOCIETY would not only afford this advantage, but would in other respects be attended with the most beneficial consequences."

This suggestion was not long in bearing fruit. A similar idea had occurred to Dr. Pearson a few years before; but he does not seem to have taken any steps to bring it forward amongst astronomers. At a meeting, however, held on the 12th of January, 1820, to take Baily's suggestion into consideration, a Committee was formed to frame rules and regulations, and the first meeting of members took place, on the 8th of the following month, at the rooms of the Geological Society in Bedford Street, Covent Garden, Baily acting at Secretary. Yours faithfully,

Blackheath, 1899, May 24. W. T. LYNN.

PUBLICATIONS.

A DARK COMPANION TO 70 OPHIUCHI.—Those interested in the investigation of the observed irregularities in the motion of certain double stars will find Dr. Moulton's paper * well worth studying. And as he also deals with the case of 70 Ophiuchi, we give a short *résumé* for the benefit of those observers who have given this system so much attention in recent years.

Dr. Moulton says that astronomers naturally assumed, as the cause of anomalous motions, the existence of neighbouring bodies not yet observed, being influenced by the verifications of the predictions in the cases of Uranus, Sirius, and Procyon. Such hypotheses, too, have *à priori* more probability, since the phenomena of certain classes of variable stars have proved the existence of dark bodies in the heavens. But when assuming the existence of a dark star in a binary system it is necessary to investigate the question of stability. Dr. Moulton then proceeds to investigate conditions of stability in certain restricted cases. Suppose there are two known bodies, which, for convenience, will be called the star and the planet. Suppose the observations seem to demand the existence of a satellite revolving around the planet. It is proposed to find a distance from the planet beyond which the satellite cannot make more than a limited number of revolutions without passing away from the planet to the star. If this limit is reached the motion will be considered to be not temporarily stable;

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* "The Limits of Temporary Stability of Satellite Motion, with an application to the Binary System of 70 Ophiuchi," by F. R. Moulton, Astron. Journ. 461.

if it is nearly reached, the question will be in doubt. But since the limit for certain instability is what is sought, several simplifications may be introduced in such a way that the region of temporary stability will be enlarged; that is, every time the problem is modified it will be done so that the perturbations of the motion of the satellite caused by the star will be less instead of greater. If the satellite motion is stable, the centre of gravity of the planet and satellite will describe very nearly an ellipse. Dr. Moulton assumes it strictly elliptic, and the eccentricity zero. Finally, he supposes the satellite an infinitesimal particle revolving around such a mass at the centre of gravity that its motion, undisturbed by the star, would be exactly the same as the actual undisturbed motion when both the planet and satellite are supposed to be finite, thus reducing the problem to one where two of the bodies are finite, describing circles around their common centre of gravity, and the third is infinitesimal, moving in the same plane. Dr. Moulton then deduces the equation

$$V^2 = n^2(x^2+y^2)+\frac{2M}{r_1}+\frac{2m}{r_2}-C,$$

where V = velocity of the infinitesimal body, r_1 and r_2 its distance from M, the star, and m, the planet, respectively.

By putting V = zero, a series of curves are obtained which separate those portions of the rotating plane where the infinitesimal body may move from those where it cannot:

$$n^2(x^2+y^2)+\frac{2M}{r_1}+\frac{2m}{r_2} = C.$$

For sufficiently large quantities of C this is the equation of an oval roughly elliptical in shape, around each of the bodies M and m, and of a much larger one enclosing both of the small ones. The satellite may move inside either of the small ovals or outside the large one. In all other parts the velocity will be imaginary.

For smaller values of C the small ovals enlarge, and for a definite value of C unite, forming an hour-glass-shaped figure. Under these circumstances there is an avenue for the passage of the infinitesimal body from the planet to the star. If the neck of the hour-glass is large, the passage from one oval to the other is assured, and that in a very short time, *i. e.* when the satellite is revolving in an orbit larger than that small oval which first touches the small oval around the star, the satellite motion is temporarily stable. Dr. Moulton then deduces certain tests and applies them to the particular case of 70 Ophiuchi, and finds that there is no dark star in this binary system permanently revolving under the conditions announced by Dr. See, viz., with a period of 36 years, and the centre of gravity of the companion and dark body moving in an ellipse with a major axis of 4″·55, and that the bright companion describes a curve around this centre of gravity with a radius of 0″·3.

This, of course, is no reason for discontinuing special observations, but rather the reverse.

A POPULAR ASTRONOMY *.—It is the fashion just at present, now that we are at an advanced epoch of the Queen's reign and of the century, to make retrospects and comparisons of the "now" and the "then." It occurred to us, on opening Prof. D. P. Todd's latest book, 'Stars and Telescopes,' what a striking comparison, but a somewhat trite one, could be made about books on popular astronomy. Another thought that occurred was, How difficult it must be to include all the heads of modern astronomy within a moderate-sized volume! But Prof. Todd seems to have managed to do it. To quote from the publishers' announcement:—

> Prof. Todd's new work is practically a compendium of astronomy in all its branches. It gives full and accurate information on all important phenomena of the heavens, and makes use in this of the latest discoveries. There are chapters on "The Calendar," "The Astronomical Relations of Light," and "Solar Physics"; also one on "The Cosmogony," giving Prof. See's new theory of cosmic evolution. Besides these and other more familiar discussions, the book contains the story of the great telescopes, and the progress effected by them, together with sketches of noted Astronomers; and the reader who desires to go to original sources will find these indicated in bibliographical lists, which follow each chapter. The illustrations are very numerous, and being as varied as the text, have great interest. In view of the wide field covered, this book will prove, it is believed, a model of completeness and scholarly statement.

This promise appears to us to be accurately fulfilled. The method of arrangement is ingenious and convenient; the general thread of the story is given in the text, and special subjects are treated of in copious footnotes, which are actually short treatises on the point at hand. For example, the chapter on the solar system has a long footnote giving the history of the researches of Lagrange, Laplace and others, on planetary motion, which is very instructive to one who wants to know about this easily. We must not forget to add that the book is founded on the ninth edition of Mr. Lynn's 'Celestial Motions,' which has been taken as the skeleton plan, and the first picture in the book is a portrait of our valued contributor.

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* 'Stars and Telescopes.' By David P. Todd, M.A., Ph.D., Professor of Astronomy and Director of the Observatory, Amherst College. With maps and 219 illustrations. Little, Brown, & Co., Boston. Cloth, gilt top, $2.00.

NOTES.

Comet Notes.—Swift's Comet has been a very conspicuous object during May. Dr. Holetschek, of Vienna, described it on May 7 as visible to the naked eye, appearing like a hazy star of the 3rd magnitude. The diameter of the head was 4′ or 5′. The tail was notably fainter than the head, but could be distinctly seen to a distance of more than 1°. Dr. Smart described it on May 23 as a slightly oval nebulosity with a small nucleus, tail not visible. It appeared much fainter than a fortnight earlier, but this was partly owing to moonlight and a milky sky. The comet should still be readily visible during June, but its brightness is diminishing pretty rapidly.

The following improved elements are by A. Stichtenoth, from observations on March 4 (Lick), March 28 (Geneva), May 5 (Hamburg) (Ast. Nach. 3567):—

$$\begin{array}{lll} T \ldots\ldots & \multicolumn{2}{l}{1899 \text{ April } 13{\cdot}0149 \text{ Berlin M.T.}} \\ \omega \ldots\ldots & 8^\circ\ 41'\ 30'' & \multirow{3}{*}{$\left.\begin{array}{l}\\ \\ \\ \end{array}\right\} 1899{\cdot}0.$} \\ ☊ \ldots\ldots & 24\ \ 58\ \ 15 & \\ i \ldots\ldots & 146\ \ 16\ \ \ 8 & \\ \log q \ldots & 9{\cdot}51379 & \end{array}$$

Ephemeris for Berlin midnight.

	R.A.			N. Dec.			R.A.			N. Dec.	
	h	m	s	°	′		h	m	s	°	′
June 3....	17	17	31	54	6	June 13....	15	17	16	38	33
4....	16	58	46	52	44	14....	15	11	6	37	7
5....	16	41	49	51	14	15....	15	5	33	35	45
6....	17	26	36	49	41	16....	15	0	32	34	25
7....	16	12	56	48	3	17....	14	55	58	33	9
8....	16	0	49	46	25	18....	14	51	49	31	57
9....	15	50	4	44	46	20....	14	44	38	29	41
10....	15	40	27	43	10	24....	14	34	11	25	24
11....	15	31	51	41	35	28....	14	29	54	20	30
12....	15	24	10	40	3						

Tempel's Second Periodic Comet (1873 II.) was discovered on its return by Perrine at the Lick Observatory on May 6. The correction required by Schulhof's search ephemeris in Ast. Nach. 3554 was only $-6^s{\cdot}4$ in R.A., and $-9''$ in Declination, a result reflecting great credit on his computations. We continue the ephemeris (for Paris midnight):—

	R.A. h m s	S. Dec. ° '		R.A. h m s	S. Dec. ° '
June 1....	19 34 17	3 53	June 17....	19 57 35	5 33
5....	19 40 19	4 5	21....	20 3 7	6 22
9....	19 46 12	4 25	25....	20 8 31	7 23
13....	19 51 57	4 54	29....	20 13 48	8 37

The comet is described as faint, but its brightness is increasing pretty rapidly.

The following are Schulhof's elements:—

Epoch of osculation 1899 June 18·0 Paris M.T.

M........	352° 26′ 23″
π	306 33 52
☊........	120 57 57
i	12 38 56
ϕ	32 49 41
μ	671″·885
log a	0·481808

Perihelion Passage 1899 July 28·51 Paris M.T.

A. C. D. C.

OBITUARY.—C. L. PRINCE. Charles Leeson Prince, to whom, as our readers are aware, we have been indebted from time to time for some very interesting communications, died, after a somewhat protracted illness, on the 23rd of April last, in the 78th year of his age. He was trained to the medical profession, and qualified as a Member of the Royal College of Surgeons in 1843, but accepted a registrarship at Uckfield, from which place he removed in 1872 to Crowborough, the elevated position of which rendered it particularly suitable for astronomical observations, in which he always took a deep interest. In 1855 he became possessed of a refracting telescope by Tulley, which had formerly belonged to Dr. Pearson and afterwards to Capt. Noble, and has an objective of 6·8 inches aperture and 12 feet focal length*. He was elected a Fellow of the Royal Astronomical Society in 1857, and communicated on various occasions to their 'Monthly Notices' papers on cometary, stellar, and other observations. His reading was extensive on the early history of the science, and he published in 1882 a translation of portions (chapters xviii. & xxii.) of the 'Machina Celestis' of Hevelius. He studied the details of the course of discovery in the nature of Saturn's ring-system, and took part in investigations initiated by Mr. Lynn, which resulted in showing that Cassini and not Ball was the first to detect the principal division in the ring.

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* There is a Report on this telescope by a Committee of the R. A. S. in vol. ii. of the 'Memoirs,' p. 507.

In 1882, also, favoured by the elevated position of Crowborough, he succeeded in observing the ingress of Venus on the Sun's limb at her transit over his disc on the 6th of December in that year, when the state of the sky over a large part of this country prevented the phenomenon from being visible, causing much disappointment at Greenwich and other places.

But, careful and assiduous as were Mr. Prince's astronomical observations, he will be best remembered by the long and unbroken series of meteorological observations which he carried on, first at Uckfield, and afterwards at Crowborough, distributing annually an interesting summary of these. In 1871 he published a small volume on 'The Climate of Uckfield and its Neighbourhood,' embodying the results of his observations, and in a second edition, which appeared in 1886, these are continued to the end of 1885, various remarks being interspersed on the natural history of the district. His last work, a literal translation of the 'Phænomena' and 'Prognostica' of Aratus, was printed for private circulation in 1895 (the latter portion had, however, already appeared in the first edition of the 'Climate of Uckfield'—it is omitted from the second), and to this is prefixed a bibliography of the numerous editions of the author (whose quotation by St. Paul is so familiar), whilst at the end Mr. Prince reproduces, with additions, some remarks which he had given before upon local and other weather prognostics. He was an occasional contributor, it may be added, to 'Notes and Queries' and other periodicals. A widow, one son, and two daughters survive him. W. T. L.

P. T. Main. Philip Thomas Main, M.A., whose death occurred on the 5th of May last, a few days before he had completed his sixtieth year, was the second and only surviving son of the late Rev. Robert Main, who was First Assistant at the Royal Observatory, Greenwich, from 1835 (entering it with Airy) to 1860, and afterwards Radcliffe Observer at Oxford, where he died in 1878. The latter acknowledges in his preface the assistance given him by his son in the preparation of his work (published in 1863) 'Practical and Spherical Astronomy,' for University use; and Mr P. T. Main brought out, in 1865, a separate smaller work on 'Plane Astronomy,' intended specially for the three-day examination. But in later years he turned his chief attention to chemistry, and held for a long time past the post of Superintendent of the laboratory at St. John's College, Cambridge. He died unmarried, but lamented by a large circle of friends.

W. T. L.

Kodak Film in Spectroscopic Photography.—Sir Norman Lockyer has lately been experimenting with flexible film, with the idea of adapting it to spectroscopic photography. The large concave Rowland grating which Sir Norman Lockyer is now using for his solar spectroscopic photographs has 20,000 lines to the inch ruled on its surface, and is of 21½ feet radius, giving a spectrum of 30 inches long. The focal plane of this grating is of necessity considerably curved; in fact the plane of accurate definition at the edges of the field is about 0·5 inch in front of the similar plane at the centre. It is therefore impossible to get a sharp photograph of the whole spectrum on a glass plate; in fact not more than 18 to 20 inches of the spectrum can be brought into focus on the same plane. This difficulty is, of course, got over by the use of a flexible film, which can be bent to the curvature of the field.

The Kodak Company has sent us a print of one of Sir Norman's photographs taken with this Rowland concave grating. This is probably the longest solar spectrum photographed at a single operation. It is 30 inches long, and shows the arc spectrum of iron with a comparison spectrum of the Sun from wave-length 3600 to 5200. Sir Norman Lockyer also hopes that by using films instead of glass plates at the next solar eclipse he will be able to obtain a greatly-increased number of photographs, owing to the rapidity with which the film can be shifted in the short space of time available for photographic operations.

An apparatus for carrying films is also being designed which can be adapted to the spectroscopic cameras at present in use in the laboratory and observatory.

A NEW FORM OF ACHROMATIC TELESCOPE.—We have received a book from Messrs. Teubner, of Leipzig, entitled 'Die Medial Fernrohre,' by Herr Schupmann, being a treatise on a new kind of telescope, which is a combination of the refractor and the reflector. The light from the object passes through a lens, then through a lens of a different kind of glass placed near a curved mirror. It is then reflected by the mirror, and passes again through the second lens. The author discusses the optics of the system at some length, and shows that the image is achromatic and loses little in brightness by the successive refractions and reflection; but we must refer our readers to the book itself for details.

A NEW STAR IN SAGITTARIUS.—In 'Harvard Circular' No. 42, dated March 14 last, Prof. Pickering reports the discovery of a Nova found in examining the photographic plates of star-spectra known as the Draper Memorial Plates. The approximate position of the star is R.A. $18^h\ 56^m{\cdot}2$, Dec. $-13°\ 18'$. The star does not appear on plates taken in the years 1888 to 1897, but was shown first on a plate taken 1898 March 8, when its magnitude is estimated as 4·7. On 1898 April 3 its magnitude is estimated as 8·2, at which magnitude it approximately remained during the month. The plates of the region which were taken later in the year have not yet been examined, but on March 13 of this year, after its discovery on the plates, it was examined visually by Prof. Wendell, when it was found to be of magnitude 11·37; its light was found to be monochromatic, with a faint continuous spectrum, from which it is inferred that the Nova, like others that have preceded it, appears to have changed into a gaseous nebula. It is stated that the star may be considered to be a Nova rather than a variable, because, as in Nova Persei, Nova Aurigæ, Nova Normæ, and Nova Carinæ, the line Hϵ is bright, while in variable stars of long period this line is always dark, being probably obscured by the broad calcium line H.

Prof. Pickering points out that during the last four centuries fifteen stars have appeared which are commonly regarded as Novæ, and that these stars are in general near the central line of the Milky Way. Their average galactic latitude is 11°·2, while if uniformly distributed in the sky it would be 30°.

THE STONYHURST OBSERVATORY.—We have received Father Sidgreaves's neat little volume for the year 1898. Besides the records of magnetic and meteorological observations, there are some notes on comparison of variation of Sun-spot areas and the spectrum of the Sun in the HK region. "The calcium radiation K being denoted by increasing intensities 1 to 4, and the apparent areas of spots expressed in units of one five-thousandths of the solar disc or circular area, we have on 21 days of no spots mean intensity of K 1·7; on the 12 days of spot area from 1 to 3 units inclusive, mean intensity of K 1·7, and on 11 days of spot-area from 4 to 6 units, mean intensity of K 2·3." From a comparison of the spotted area with the simultaneous disturbance of the magnets, it seems that "there is at present no clear law connecting the magnetic disturbances with the Sun-spot areas of the year"; but this could scarcely be expected to be shown from one year's observations.

OXYGEN IN HELIUM STARS.—The following note is published by Sir William Huggins in Ast. Nach. No. 3565:—"In putting together for early publication photographs of star-spectra taken here I have found a confirmation of Mr. McClean's statement as to the presence of lines of oxygen in stars characterized by strong helium lines.

"In the region of the spectrum beyond K, about ten lines given by Neovius as belonging to oxygen appear to coincide with lines in the spectra of Rigel, of β Lyræ, and of some other stars. The relative intensities of the stellar lines do not in all cases agree with those given to the lines of oxygen by Neovius.

"These observations appear to be of sufficient interest to justify this preliminary note in anticipation of a fuller account which will appear in the book itself."

A PUBLIC OBSERVATORY IN LONDON.—In the 'Daily Telegraph' of April 26 it is announced that a resident of Hampstead has offered a telescope and observatory to the London County Council, if that body would provide a site and take charge of it generally for the public benefit. The L. C. C. did not see its way to incurring the responsibility; but a few private individuals have constituted themselves Trustees, and hold the instrument pending the supply of a suitable site.

A REMARKABLE DAYLIGHT APPEARANCE OF VENUS.—The following incident is told in a cutting from a United States newspaper, kindly sent us by a correspondent. In El Paso, Texas, which is called the Lone Star State, it is customary on public holidays to raise the "Lone Star" flag on a certain public building. On March 2 last, Texas Independence day, at 10 o'clock in the morning the rope supporting this flag broke and the flag fell. Eyes being naturally turned skyward, some one and then others saw a lone star in the sky, presumably Venus, which remained visible until 4 o'clock in the afternoon. Apart from the coincidence, it is remarkable that Venus should be visible to the naked eye for such a long time in daylight. That the circumstance actually happened as related is vouched for by certain persons who took oaths to that effect before a notary, who in his turn certified that *they were sober and entitled to credit*, an unusual addendum to the record of an astronomical observation.

SIR EDWIN ARNOLD, in the 'Daily Telegraph' of May 27, writes as follows:—"There is an American Professor roaming the globe in search of meteorites to enrich his now almost unique collection, at Rochester, U.S.A., of these mysterious visitants from space. Let no matter-of-fact person lightly mock his passion, for it is, perhaps, to meteorites that we owe the strongest proof available that organic life exists in the regions whence come shooting-stars. In an upper gallery of the South Kensington Museum may be seen a small glass tube, containing a yellowish powder, which powder is hydrocarbon extracted from a fallen sky-stone. Now, hydrocarbon is what may be more familiarly called pie-crust, and cannot be composed except by the inter-agency of vegetable or animal life; so that same little pinch of yellow dust is almost a demonstration in itself of far-off stellar life, which has somewhere blended the hydrogen and carbon into organic substance. Strange, all the same, that the best proof extant of angels and infinite sentient existence should be a bit of pulverized dough inside a meteorite!"

DOUBLE STARS.—Dr. Eric Doolittle, in the 'Astronomical Journal,' No. 460, gives corrected elements of the orbit of Hercules. He makes the period 34·547 years and semi-major axis 1″·38. The predicted places differ considerably from those given by the orbits of Dr. See and Dr. Doberck. The Astronomer Royal permits us to state that the predicted place for 1899·50 is in close agreement with that actually found at Greenwich.

MR. MAUNDER ON CORONAL PHOTOGRAPHY.—On April 11 last Mr. E. W. Maunder lectured before the Royal Photographic Society on Exposures in Coronal Photography, The Earl of Crawford, President of the Society, occupying the Chair. After reviewing and showing specimens of the work of several observers during the 1898 Eclipse, the lecturer gave, as a deduction from these, his opinion that it is desirable to make either very short or very long exposures during an eclipse, as was stated at the meeting of the B. A. A. reported in our last number. Capt. Hills and various other persons joined in the subsequent discussion.

A NEW REDUCTION OF PIAZZI'S STAR OBSERVATIONS.—It is stated in 'Popular Astronomy' for May that Dr. H. S. Davis has resigned his position on the staff of the Columbia University Observatory, New York, and will now devote himself entirely to the reduction of Piazzi's observations, on which he has been partly engaged for some years. It may be recalled that Piazzi made about 125,000 observations between the years 1792 and 1813, with a meridian-circle and transit-instrument; it is these which are being re-reduced with the best available constants now known. Prof. Porro, of Turin, has undertaken the work relating to 34,000 of these, and Dr. Davis makes himself responsible for the remainder. The result will be a valuable catalogue of about 8000 stars for epoch 1800. These stars are also being re-observed; their places at epochs separated by 100 years should be a valuable contribution to fundamental astronomy.

We call attention to the Partial Eclipse of the Sun which will be visible in the British Isles on June 7 next (early morning of Thursday, June 8, civil day). The circumstances will be found in the Companion. The eclipse of greatest magnitude in Great Britain is represented by only 0·263 of the diameter, so that it will not be a striking phenomenon.

There will also be a Total Eclipse of the Moon on June 22–23 G.M.T., invisible at Greenwich.

In the number for January last, p. 69, we mentioned that the people of Adelaide, South Australia, were dissatisfied with the working of the zone time-system, so far as it referred to them. It is announced that, in accordance with the amended Standard Time Act, Adelaide time was advanced half an hour on May 1, so that the time of South Australia is now 9½ hours fast on Greenwich.

It is announced that Prof. F. L. O. Wadsworth, who has been connected with the Yerkes Observatory from the beginning of its work, has resigned his position on the staff. His present address is 283 Lincoln Avenue, Bellevue, Pa.

A bronze tablet has been placed on the house, No. 19 New King Street, Bath, where Sir W[illegible]m Herschel lived, recording that fact. This was unveiled by Sir [illegible]bert Ball on April 22 last, and the occasion was made of some little ceremony.

A committee of the St. Petersburg Astronomical Society has been appointed to consider the question of the reform of the Russian Calendar. It seems likely that before long Russia will be in accord with the rest of Europe in the numbering of days.

We have lately had the opportunity of reading a most interesting little treatise on cyclones, illustrated by cyclone-tracks as observed in various years, the substance of a lecture delivered by Mr. T. F. Claxton, the energetic Director of the Royal Alfred Observatory, Mauritius, an island where this subject is naturally of much interest.

We are glad to hear that Professors Hussey and Aitken are making great efforts at Lick to re-observe the whole of the Otto-Struve stars. Many of these pairs have been greatly neglected.

M. **Solá** publishes a third series of double-star measures (about 50 pairs) in A. N. 3563.

Erratum.—In the April number, p. 166, line 38, for "Palm Sunday" read "the eve of Palm Sunday."

From an Oxford Note-Book.

Among those who have recently done important work in planetary theory, it is somewhat of a puzzle to give Gylden his proper place, as has been remarked before in these notes. Herr Schultz-Steinheil, of Lund, in working at the perturbations of the planet Alexandra (54) by Jupiter tried first Gylden's methods, but found the series converged very slowly, and was compelled to break off the work and attack the problem by Hansen's method, at any rate in the first instance. It seems possible that we may have to wait some time to find out the exact practical value of Gylden's methods.

A geodetic survey of the greatest importance is just being undertaken. In a recent letter (April 17) Dr. Gill announced his departure to spend a month at Gwels, 150 miles east of Bulawayo, "to start the arc of meridian on the 30th meridian, which I hope to terminate at Cairo. It is sanctioned as far as latitude 15° S." Perhaps the following paragraph from a paper read by Dr. Gill at the Graham's Town Exhibition, in December last, will give concisely an idea of the present state of things ('Cape Times,' Dec. 23, 1898):—

> In 1897 the geodetic survey of Southern Rhodesia was begun, and is now well advanced from Bulawayo to Salisbury. I am now empowered by Mr. Rhodes to state here to-night that as soon as he has made the financial arrangements to begin the work of railway construction from Bulawayo to the Zambesi, he will place at my disposal the funds necessary to carry the geodetic operations from the south of Rhodesia to Lake Tanganyika. I think that our friends the Germans will not be slow to carry on that triangulation through their territory along the east side of Lake Tanganyika to Uganda, and now that Lord Kitchener has so effectually and brilliantly cleared out the Mahdi there is a fair field for British science and British energy to carry on the work from Uganda to Alexandria. We should then have a direct arc of meridian from the Cape of Good Hope to latitude 18 deg. south. Another arc from latitude 22 deg. south in Rhodesia to 31 deg. north at Alexandria, and then we have Struve's great arc of meridian extending from latitude 45 deg. N. in the south of Russia to 70 deg. 40 min. at the North Cape, and the whole of this nearly along the meridian of 30 deg. E. of Greenwich.

CONFIRMATION of Mr. McClean's discovery of oxygen in the spectra of β Crucis and other stars seems to be accumulating. In Ast. Nach. 3565 Sir William Huggins says:—"In putting together for early publication photographs of star-spectra taken [at Tulse Hill], I have found a confirmation of Mr. McClean's statement as to the presence of lines of oxygen in stars characterized by strong helium lines." The announcement of the confirmation is as welcome as that of the early publication of the new book, which will be awaited with the greatest interest.

In the same number of the Ast. Nach. Sir J. N. Lockyer says that his own observations, so far as they have gone, tend to confirm Mr. McClean's view, but other photographs and more laboratory work are needed to explain certain changes of intensity which have been observed. Dr. Gill has also obtained evidence of equal importance.

As regards his own conviction of the presence of oxygen, Mr. McClean probably stood in no need of independent evidence; but it is nevertheless always pleasant to have a discovery accepted and confirmed by others, especially when there is a little delay before such acceptance. The announcement of the presence of oxygen in the spectrum of β Crucis was made in 1898 February; but a year later, in his Presidential Address, on presenting the Gold Medal to Mr. McClean, Sir R. S. Ball was only able to refer in a guarded manner to the discovery, owing to the deficiency of independent evidence. This deficiency seems now in the fairway towards removal, and I think we may accordingly congratulate Mr. McClean afresh.

THE following paragraph from an American newspaper was kindly sent me by the Director of a great American observatory, but without any such weighty recommendation I think it is worthy of a place here. (I fancy the date may be a year or two back, as it was found among "odds and ends" by the sender.):—

MATHEMATICAL GENIUS SNUBBED.

The Indiana Senate kills a Bill Demonstrating a New Mathematical Truth—How to Square the Circle.

[Special Telegram to The Dispatch.]

INDIANAPOLIS, Feb. 14.—A bill "demonstrating a new mathematical truth," as it is termed in the title, or, in common language, how to square the circle, was recently introduced in the Legislature by Mr. Record, of Posey county, and attracted a good deal of attention. It contained a long mathematical direction of a geometrical and algebraic character, and when it was first read in the House it created a great deal of merriment and was looked upon as a joke. The Speaker facetiously referred it to the Committee on Swamp Lands, and later changed the reference to the Committee on Canals.

The latter committee looked it over and, concluding that there was more in it than appeared on the surface, referred it to the Committee on Education. The latter brought in a report recommending its passage, and it was passed unanimously by the House last week.

The author of the bill is Dr. Edwin Goodwin, a mathematician, living in the southern part of the State. He has submitted his demonstration to a great many eminent mathematicians, and some of them conceded that it is the solution of the long undiscovered problem of how to square the circle.

The basic principle of the demonstration is that the established ratio, 3·1416, should be 3·2. The object of the bill was to secure the indorsement of the State Legislature as an advertisement, in return for which the author proposed to allow the free use of the demonstration in the public schools. It was transmitted to the Senate, where a vote was taken yesterday, killing the bill.

From America also comes a circular directed against aberration, by Leopold Eidlitz. It is such a beautiful example of the workings of a paradoxer's mind that I feel quite sorry not to be able to quote it in full, and can only hope that most of our readers have had a copy. The following paragraph, as a specimen of the style, will be sufficient to show the copyless what they have lost:—

> Furthermore, assuming the definition of the so-called constant of aberration, in accordance with the theory accepted at present, to be correct (20″·5), let a telescope one hundred feet long (the object glass being three inches in diameter) be pointed towards the supposed image of the Sun in the direction of the resultant (of the velocities of the propagation of light and that of the motion of the Earth in its orbit around the Sun). Let it be further supposed that an image of the Sun is then visible, there is no optical reason imaginable why the real Sun in its true position could not also be seen at the same time, or that the image presented in the telescope would not be an oval Sun, the horizontal diameter of which is to its vertical diameter as 11 to 10 nearly. That we do not see the Sun in that form disproves Bradley's theory of aberration. The oval Sun thus seen would occupy 1·28 inch of the field of vision and would be 0·06 inch out of centre of the same.

EDITORIAL.

The following announcement has recently appeared in a Blackheath paper. We feel sure that astronomers all over the world will join in wishing happiness to the author of our Oxford Note-Book and his bride:—

> The marriage between Mr. H. H. Turner, M.A., F.R.S., Savilian Professor of Astronomy, Oxford University, and Agnes Margaret, eldest daughter of Mr. Robert Whyte, Pentland House, Old Road, Lee, has been fixed to take place at St. Margaret's, Lee, on Saturday, the 17th June, at 2.30.

THE OBSERVATORY,

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1899 June 9.

Prof. G. H. DARWIN, LL.D., F.R.S., *President*, in the Chair.

Secretaries: H. F. NEWALL, M.A., and F. W. DYSON, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Dyson. 76 presents have been received since the last Meeting. One of these of great interest is volume viii. of the 'Publications of the Astrophysical Observatory of Potsdam,' which is the first publication by any observatory of the measures made for the Astrographic Catalogue. We have received also from Mr. Lynn a book by Mr. Robert Brown 'On the Origin of the Primitive Constellations,' vol. i.; and Prof. Schur has sent us a volume entitled 'Wilhelm Olbers, Leben und Werke. Neue Reduktion der Beobachtungen von Kometen und kleinen Planeten.'

Dr. Johnstone Stoney presented a paper communicated by himself and *Dr. Downing* jointly, giving ephemerides of the ortho-Leonids. The dense part of the great procession of meteors will probably be streaming across the Earth's orbit next November. The Earth will reach the node probably not far from the epoch 1899 November 15^d 18^h (see 'Proceedings of the Royal Society,' vol. 64, p. 409). At that time, were it not for moonlight, an end-long photograph of the part of the stream which is near the Earth but outside the atmosphere might perhaps be secured by pointing the telescope along the tangent of the meteoric orbit, which would require it to be pointed towards a situation in the heavens some degrees distant from the radiant-point. But as the Moon will be nearly full it seems hopeless to obtain this photograph.

Under these circumstances, and as Dr. Isaac Roberts expressed his willingness to make another attempt to photograph the

Leonids while outside the Earth's atmosphere, the best course appeared to be to provide an ephemeris which will enable observations to be made on groups of nights upon which the Moon will not interfere, and which come near the date on which the Earth reaches the node. This is done in the subjoined ephemerides. The Earth is so situated on those dates that an actually tangential view of the stream cannot be obtained, and accordingly we had to be content with computing the ephemeris for situations in the stream where it makes a small angle with the line of sight. Two such situations were selected, one to be observed in the group of nights free from moonlight which come next before November 15, and the other to be observed in the group of dark nights following that date.

The computations were made by Mr. Thomas Wright, of the Nautical Almanac Office, and the expense of computation was defrayed out of the Donation Fund of the Royal Society.

Dr. Roberts. Astronomers everywhere, particularly those connected with this Society, must be much indebted to Dr. Stoney and Dr. Downing for the trouble, thought, and energy they have devoted in furnishing ephemerides with the object of enabling us to photograph the meteor-stream, and if we do not succeed in accomplishing that object the value of their labours will in no way be diminished. They have utilized the best data that were available to them by the observations of 1833 and 1866, and if we are able to verify their calculations much will be gained in the advancement of science. Every endeavour shall be made in November next to obtain the most accurate data possible, in order to determine the orbit and the movements of these meteors for future use.

Most people will understand the difficulty in photographing objects so small and emitting so little light as these meteors are proved to emit whilst they are moving across the photographic field, and unless several of them are overlapping when the telescope is pointed to their positions, so as to combine the light of a number of them over a considerable interval of time as it impinges upon the photographic plate there is little chance that they will leave a record upon it. If we could point the telescopes to the meteors as they come end-on to the field, the probability would be much greater that there would be an image on the plate; but still it would depend upon the number of them following each other so closely that their images would overlap and so give sufficient time for their having effect on the plate. The successful application of the photographic method is what we most desire to see, and I hope when the time comes we shall be favoured in some degree.

The President. When you made your abortive attempt to photograph the meteors I presume you had not a complete ephemeris before you.

Dr. Roberts. Yes; the ephemeris extended from the 24th of

December to the 1st of March, and any one of the photographs which were taken on the recorded three clear nights would have proved the photographic effects of the meteor-stream. The sky during the exposures of the plates was as good as could be expected, but still the results were negative.

Dr. Stoney. On both of the occasions, in the spring of this year and in the spring of 1898, when Dr. Roberts made the attempt to photograph the Leonids in the open sky, the direction of view was not nearly so tangential as it will be in the situations for which these new ephemerides have been computed. There will, on this account, be a more favourable opportunity on the coming occasion, and, moreover, the meteors will be a little closer to the Sun, and therefore a little brighter. With reference to what Dr. Roberts said about the objects of the stream being sufficiently close to overlap, they are certainly much closer than is sufficient for their images on the photographic plate to overlap, and that is sufficient. Even under the most favourable circumstances the chance of getting the photograph may be small, but it seems an astronomical duty to make the attempt, and if Dr. Roberts can succeed in getting a photograph of these orbs while in the open sky he will furnish us with data for correcting the orbit better than can be obtained from the only observations at present available—those made after the meteors reach the Earth's atmosphere. I may take this opportunity of pointing out that when determining the radiant-point from photographs of the trails of luminous meteors, it will be necessary to make allowance for the fact that, after entering the Earth's atmosphere, the speed of a meteor decreases and its path becomes curved.

The President. There is one point I have not heard you speak of. Regarding each meteorite as a small planet, I assume it is sunlight that is the source of light. Is it not essential that their phase should be considerable?

Dr. Stoney. Quite so; the phase must be considerable. In the observations for which the ephemerides have been prepared the phase of each meteor will be nearly the same as that of a half-moon, since the positions in the stream to which the camera will be directed will stand from the Earth nearly at right angles to the direction of the Sun.

The President. Are not the special perturbations due to the Earth very considerable? Had these been taken into account?

Dr. Downing. No, they have not been.

The President. The thanks of the Society and of all astronomers are due to the authors of this paper for the labour they have taken, and we can only hope that Dr. Roberts and other astronomers will be successful next autumn, notwithstanding their previous failure.

Mr. Dyson then read a paper on further investigation concerning the Position-error affecting Eye-estimates of Star-magnitudes, by *Mr. A. W. Roberts.* He remarked that Mr. Roberts, of Lovedale, South Africa, had communicated a previous paper in the 'Monthly Notices,' vol. 57, on the same subject. He had been continuing his work, confining his observations to eye-observations and not telescopic observations. The two stars chosen were ζ and λ Pavonis. Mr. Roberts had investigated this in considerable detail, and had made numerous observations in comparing the magnitude of the stars. The most interesting thing about the paper was the very large size of the coefficient. It seems remarkable that a variation amounting to one magnitude should result from this cause.

Mr. Seabroke. How far are the stars apart?

Mr. Dyson. About 10 or 12 degrees.

Mr. Seabroke. I asked this because it seems to me that there is a limit to the distance within which one can see two objects without moving the eye.

Capt. Noble. It is frequently stated that high stars and low stars require a different adjustment of the eyepiece for focus. I may say, to my eye, that both high stars and low stars require the same focus.

The President. I cannot but think that any comparison with the eye really fixed is out of the question. I do not know whether anyone has read Helmholtz's 'Physiological Objects.' Helmholtz tried a number of tricks with his eyes. If anyone has ever tried the experiment he will be surprised at what a short distance from the central point of vision details fade away. I cannot consider that any man, endeavouring to look at two objects, ever keeps his eye still. I think he keeps his eyeball moving, and carries the recollection of what he sees from one to the other, for the portion of the eye which has very distinct vision is in fact a very small one. I know that to my cost, as I had a very severe blow on the eye many years ago, and it has made me perfectly blind to detail with that eye. I do not think Mr. Roberts claims to have effected his comparison with the eyeball held still. It is a curious and interesting subject, and our thanks are due to Mr. Roberts for his communication.

The President. We have present this evening some very distinguished astronomers—Prof. Simon Newcomb, of Washington, and Prof. Barker, of Philadelphia, and M. Deslandres, of Paris. I am sure we should all value some words from Prof. Newcomb, so I will ask him to tell us something of astronomy across the Atlantic.

Prof. Newcomb. I must thank you for this very warm greeting, which I am glad to say is not the first I have had from this Society. I am afraid I have no important scientific communication to make, but you may possibly be interested in some remarks on a subject in which this Society plays a very great part. In the year 1877 I had the honour of presiding over the American Association for the Advancement of Science at Nashville. During that time I was informed by the people of that city that there was a young man, a little over 20 years of age, who was a photographer, and had a reputation as a coming astronomer. With his scanty earnings he had bought a small telescope, and with it was trying to explore the heavens. I expressed a wish to see him, and found he was very desirous of doing something that might be useful in astronomy, and wanted to know if I could not tell him what that something should be. I suggested that there was an unexplored field of work in the photography of comets. I never discovered a comet myself, and my ideas were rather theoretical, but I gave him the best instruction I could and we parted. For the remainder of my tale we must go ahead 18 years. About two years ago the Gold Medal of this Society was awarded to that same young man. He came across the Atlantic to receive it, but the ship in which he came encountered extraordinarily bad weather, and the result was at the moment when the Medal was being awarded in this room he was passing up the Solent, and he arrived here a day after the fair. You, however, did not deprive this man of the pleasure of being received by this Society on account of the delay which happened to him, and accordingly, I understand, you organized an additional Meeting, where you had Prof. Barnard present. I must assure you that the circumstance of that extra Meeting being arranged gave a great deal of pleasure in America, and I wish to express my high appreciation of the spirit which was shown on that occasion.

The President. It should need no words of mine to inform Prof. Newcomb how high is our appreciation of American astronomers. I understand that M. Deslandres is willing to offer us some remarks on his work at Meudon.

M. Deslandres gave an account with some detail of the large telescope and the coelostat that are being made for the Paris Exhibition of next year.

The President. I am sure the Society will have been greatly pleased to hear the latest news about these large instruments. We thank M. Deslandres for his communication.

The President then called upon Prof. Barker.

Prof. Barker. I am rather sorry, Sir, that your eyes are not a little poorer than you have told us they are, as I might then have had the benefit of absence without the actual fact. I came to the Astronomical Society on the invitation of my friend Sir Robert Ball, and I wish to say that I have very much pleasure in being present at a Meeting of the Society of which I have heard so much on the other side of the water, and the work and membership of which we value so highly. As you say, my work has been not only in the department of physics, but also in the direction of chemistry. We workers on the physical side all feel as did Foucault when he published his researches on the velocity of light. "I do not offer this," said he, "as supplanting astronomical methods, but as supplementing them. I only offer it to show that it may be possible to determine the speed of light by physical methods." It was a great gratification to us in America—and, I have no doubt, an equal gratification to astronomers all the world over—when Prof. Newcomb applied to Congress for an appropriation to enable him to determine the velocity of light by a physical method. The time had come when physical experiments had so enormously increased in accuracy that it became possible to determine the solar parallax with an approximation which was probably equal to the methods of astronomy. I think the reasons which led the National Academy of Science at its recent meeting to award its Medal to Dr. Keeler, the present Director of the Lick Observatory, have been a matter of congratulation everywhere. It seems to us that this medal was awarded in a great measure for that extraordinary piece of work of his on the determination of the constitution of Saturn's ring by the spectroscope, and that it was thoroughly deserved. I desire to express my pleasure at meeting the Royal Astronomical Society.

The President. There is present here to-night a gentleman who is, in one sense, our junior Fellow, for I have had the pleasure of admitting him to-day; I mean Mr. Berthon: but I may remind you that he may also be described as our senior Fellow, for he was a Fellow of the Society during many years. He has no paper to read this evening, but he has a dividing-engine and a portable telescope which he would like to exhibit to the Fellows.

Rev. E. Berthon. I suppose that in age I am the oldest Member of the Society, being in my 87th year. 40 years ago I was a Member, but I had rather painful reasons for giving it up. I am not able to add anything scientific to the glorious labours of this Society, but I have been working a great deal in the way of making improvements in astronomical instruments, and I think you will be glad to know that the difficulty of accurately dividing circles has been in some degree remedied by what I have done lately. (Mr. Berthon proceeded to show his dividing-machine, the main point of which was a screw cut on an *annular* rod, around which a nut ran, without looseness or backlash; and also a portable equatorial telescope.)

The President. Our thanks are due to Mr. Berthon for his communication. We must all be struck with the versatility of Mr. Berthon, whose church has one of the most beautiful Norman naves in England, who is the inventor of the Berthon boat, used on mail-steamers all over the world, and who has now come here to-night to show us how to divide circles.

Mr. Newall gave an account of a paper communicated by *Mr. Hinks*, of the Cambridge Observatory, on the Construction and Use of Réseaux. The point that the author brought forward was that, whereas the usual method of printing was to attach the réseaux and plate to the object-glass of the telescope with the luminant at the focus, he had found it advisable to dispense with the use of the object-glass. To do this it is necessary to have the lines in the silver cut wider than was usual. The plate could then be put further from the réseau, which is an advantage, as the réseau would not deteriorate so rapidly.

Mr. Hollis. I do not quite understand, from the description we have heard, whether the printed lines are wider than they are when the réseau is used in the usual way. If this is so, the saving of réseaux would, I think, hardly compensate for the extra time that would be necessarily spent in measuring. In the early days of the astrographic work at Greenwich we used to have broad lines, but the saving of time and the increase of accuracy in measuring which result from the use of fine printed lines are immense.

Mr. Dyson. I think that the point of the paper is that, though the lines cut in the silver are broader, the printed lines are narrower. Also it is made possible to use plates on ordinary glass, which cost fourpence, instead of plate glass, which cost a shilling.

Dr. Spitta. I do not know whether it is fair to compare a microscopical image with the image of a réseau line. If I understand the paper rightly, the author says that with a larger luminant and a broader slit, a great deal of the diffraction is got rid of. In the case of microscopic observation of bacteria, where the objects are exceedingly small and the magnification is very large, if a large slit of light is used, there results a much better-defined image than with a small one. This is not a discovery on my part. Koch spoke of this when he first made photographs of bacilli. It is not without interest to take a photograph of the same object with a large cone of light and with a small one. The difference is remarkable: a very much better picture is obtained with the large cone.

The following papers were announced and partly read:—

A. W. Roberts. "Further Investigation concerning the Position-error affecting Eye-estimates of Star-magnitudes."

John Tebbutt. "Equatorial Comparisons of Jupiter, Uranus, Neptune with certain Stars in Newcomb's Standard Catalogue."

W. H. Robinson. "Note on the Nebula N.G.C. 6535."

G. Johnstone Stoney and A. M. W. Downing. "Ephemeris of two Situations in the Leonid Stream."

A. E. Young. "On the Formulæ of Reduction to the Meridian of the observed Zenith-distances of Stars."

A. R. Hinks. "On the Construction and Use of Réseaux."

Royal Observatory, Greenwich. "Observations of Comet *a* 1899 (Swift)."

The following gentleman was elected a Fellow of the Society:—

Frederick Evan Peach, 161 Stanstead Road, Forest Hill, S.E.

The following Candidates were proposed for election as Fellows of the Society:—

H. C. Plummer, Owens College, Manchester (proposed by Wm. Esson).

C. J. Taylor, Port Elizabeth, Cape Colony (proposed by L. A. Eddie).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The sixth ordinary Meeting of the Session was held on Wednesday, 31st May, at Sion College, *Mr. W. H. Maw*, President, in the Chair.

The names of nine Candidates for Membership were read and suspended, and the election of seven new Members was confirmed.

Mr. Edwin Holmes read a paper on "A Year among Double Stars." He stated that during 1898 he had observed 423 double stars, many of them several times, besides a number of nebulæ, and had made occasional observations of the Moon, Jupiter, Mars, and Neptune. There were very few nights without either clouds or mist, and definition was seldom even fair. He would not have ventured to refer to his crude doings if the Association had not experienced complete failure in their attempt to form a Double-Star Section. One reason for the lack of attention to double stars was the difficulty amateurs with altazimuth telescopes experienced in finding. Another deterrent was the apparent extreme accuracy of published measures. Angles were given to a minute fraction of a degree, and distances to the hundredth of a second, and the amateur, despairing of ever achieving such accuracy, concluded it useless to attempt anything. But such seemingly exact measures were entirely misleading, and the roughest measures, treated in the orthodox way, would work out to any degree of refinement. He was afraid double stars were not so attractive to some as planets, because there was less room for the imagination.

Capt. Noble was bound to say he agreed with Mr. Holmes in what he had said about some published results—not merely of measures of double stars, but also concerning appearances alleged to exist on other objects in the sky.

The President said they were always glad to hear Mr. Holmes's criticisms of doubtful observations, and he thought there was a great deal in what the writer of the paper had said about the imaginary accuracy of double-star measures. Everyone who had done double-star work would, he thought, admit that reliance could not be placed on the second decimal place of seconds in published results, but he thought it would be undesirable to omit the decimal place in the statement of angles, as they were useful in making averages of several observations. Mr. Holmes had said that the Director of the Double-Star Section ought not to be a Fellow of the Royal Astronomical Society, but the fact was that Mr. Seabroke and he (the President) had been observing double stars for many years, and had commenced publishing their records in the Memoirs of the R. A. S., and thought it undesirable to make any change.

Mr. Chambers said he was tempted to make a remark that would probably be unpopular. Double-star observations required exceptional advantages in the way of instrumental appliances, weather, and personal capacity and experience, such as he thought comparatively few of the Members of the Association possessed. There were many other branches of observational astronomy in which Members could do useful work, and he thought it would be far better for the Association to leave to the large observatories all, or nearly all, double-star work.

Mr. Petrie read a paper by *Prof. W. H. Pickering* on "The Elongated Satellites of Jupiter." The author said that upon page 60 of the present volume of the Journal of the Association, M. Antoniadi endeavoured to explain the egg-shaped form of these bodies as an optical illusion. In referring to his (Prof. Pickering's) observations, M. Antoniadi spoke of "rapid changes in the outline of Jupiter's third satellite, which was seen alternately round and elliptical." In this statement he seemed to have confused the first and third satellites. It was the first and smaller of these bodies that presented the rapid change noted. The third satellite was only occasionally seen to be very elliptical, but when it was so, it remained of that form for from one to three evenings continuously. The reason he (Prof. Pickering) had always laid less stress on his observations of the third satellite than on those of the first was that the elongation did not always occur when it was expected, in this respect differing markedly from the case of the latter body, which had never been known to fail when the weather conditions were favourable to observation.

Mr. Maunder said Prof. Pickering himself gave in person an account of his observations when the Association used to meet in Barnard's Inn—some six or seven years ago. At that time he hazarded the theory that the ellipticity of the Satellites of Jupiter proved that the satellites were not true satellites, but were simply meteoric swarms. The observations of Prof. Barnard that had been published in the 'Monthly Notices' within the last few years and the observations Capt. Molesworth gave them at the recent Conversazione had satisfactorily explained the apparent changes in the first satellite as being due to a bright equatorial belt and dull polar regions, whilst Capt. Molesworth had shown that the apparent ellipticity of the third satellite might be caused by the presence on it of dark markings which produced, when they were on the edge of the disk, the appearance of an oval instead of a circular body.

Those who were present at the Conversazione and saw the slides which Capt. Molesworth showed, would have no difficulty in accepting the explanations which he gave of his observations, which were very similar in character to those of Prof. Pickering.

A paper on "The Relative Chronology of Lunar Formations," contributed by *Mr. Charles F. Smith*, was read, and was followed by a paper on "The Moons of Mars" by *Mr. C. T. Whitmell.*

Mr. Maunder then gave an address, illustrated by a number of lantern-slides, on the Signs of the Zodiac and other Constellation Figures. He reminded the Meeting that the blank space left in the southern heavens by the ancient charts gave them the key both to the date when the constellations were first mapped out (that is to say, some 2800 years B.C.) and the latitude (namely, north latitude 38° to 40°).

Mr. Tabor asked whether it was not probable that the signs were of pre-Aryan origin, and *Capt. Noble* remarked that some of the signs, Hercules for instance, were upside down.

Mr. Maunder replied that the signs had certainly been known to early Turanian peoples. With reference to Capt. Noble's remark, Hercules must have been upright when the constellations were mapped out.

It was announced that upwards of one hundred Members and friends had sent in their names as wishful to join the eclipse expedition of next year.

ROYAL METEOROLOGICAL SOCIETY.

The last Monthly Meeting of the present Session was held on Wednesday afternoon, June 21, at the Society's new rooms, 70 Victoria Street, Westminster, *Mr. F. C. Bayard, LL.M.*, President, in the Chair.

Dr. R. H. Scott, F.R.S., read a paper "On the Heavy Falls of Rain recorded at the seven Observatories connected with the Meteorological Office during the 28 Years 1871–98." The data were derived from the records of the Beckley self-recording rain-gauges at the following places:—Valencia, Armagh, Glasgow, Aberdeen, Falmouth, Stonyhurst, and Kew. These records have been tabulated for each hour, and it is from these hourly tabulations that Dr. Scott has extracted the heavy falls. He finds that Falmouth has the greatest frequency of heavy falls, the next station being Valencia, and then Stonyhurst. The most exceptional fall during the whole period was at Glasgow, at 5 P.M., on August 11, 1895, when as much as 0·80 in. was collected in ten minutes. The information given in this paper is likely to be of much service to engineers, who want to know the rate at which rain sometimes falls in short periods.

A paper by *Mr. J. Baxendell*, describing his new self-recording

Anemoscope, was read by the *Secretary.* This instrument, which records the direction of the wind on an open scale, has been in use at Southport for more than a year, and works very satisfactorily. The vane, which is an exceedingly light, but large double-bladed one, is sensitive even in light airs, and is steady in the strongest gales. The records from this anemoscope, which were exhibited at the Meeting, were very clear and of an interesting character, and showed the instrument to be a valuable companion to the Dines pressure-tube anemometer.

A paper by *Mr. R. C. Mossman, F.R.S.E.,* "On the Average Height of the Barometer in London," was also read by the *Secretary.* Some years ago Mr. H. S. Eaton worked out the mean monthly and annual height of the barometer in London for 100 years. Mr. Mossman has carried on this discussion for a further period of 20 years; but he finds that the results for the 120 years are practically identical with those for 100 years.

Six Months' Work with a Seismograph at the Royal Alfred Observatory, Mauritius.

A Milne's Seismograph has now been at work at the Royal Alfred Observatory for six months, recording automatically every movement of the Earth's crust, from the violent outbursts termed earthquakes to those minute undulations and tremors of which we ourselves are unconscious, but which, nevertheless, play an important part in the investigations of the geologist and astronomer.

The work done up to the present has brought to light many interesting and important facts, almost sufficient to make one no longer look upon the island of Mauritius as an inert mass of rock, but to consider it as an elastic and breathing body, whose pulsations are controlled indirectly by the Sun.

It has already been pointed out * that there is a relative upheaval to the west of the instrument from 7 A.M. to 5 P.M., and to the east of the instrument from 5 P.M. to 7 A.M., the latter exceeding the former in almost every case, indicating a residual upheaval to the east of the instrument which has assumed the form of a progressive change in the perpendicular towards the west, at present inexplicable, although an explanation of the diurnal oscillations may be found in the registers of December 5, 6, and 7.

On December 5 heavy rain began to fall at 11.50 A.M. from a cloud having little or no horizontal movement, and extending only about a mile to the east of the Observatory; the edge of the cloud was perfectly well defined, and at the Botanical Gardens (1½ miles to the east of the Observatory) no rain fell, while one inch was measured at the Observatory from $11^h\ 50^m$ A.M. to $2^h\ 20^m$ P.M.

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* 'Planters' and Commercial Gazette.' Mauritius.

The seismograph seemed to be remarkably affected by this rain; at $12^h\ 7^m$ the boom of the seismograph began to move steadily towards the east, and finally went out of range at $12^h\ 17^m$, after an easterly movement of $7''\cdot5$. It is impossible to say what the total easterly movement may have been, though, as the boom remained out of range all day and was moving very rapidly at first, it is probable that the total movement was something abnormal. On the following morning, to bring the boom back to the middle of the scale, the level screw had to be turned through $16°\ 31''\cdot12$, which is irrespective of the amount of westerly movement which may have taken place after the initial easterly movement.

On December 6 heavy local rains again occurred, extending, however, somewhat farther to eastward (about 4 miles), and the boom again moved towards the east; but this time the movement was not so steep at the beginning. From $11^h\ 45^m$ A.M. to $11^h\ 55^m$ A.M. the easterly movement was $1''\cdot2$; from $11^h\ 55^m$ to $12^h\ 3^m$ it was $0''\cdot75$; and from $12^h\ 3^m$ to $12^h\ 12^m$, when the boom went out of range, the movement was $5''\cdot97$.

On the third day, December 7, local rains of a somewhat different character occurred, the principal falling to the south with moderate rains from overhead to about two miles east of the observatory, while farther eastward the weather was fine.

On this occasion the boom moved towards the west with moderate rapidity at first, but more gradually afterwards; from $0^h\ 20^m$ P.M. to $4^h\ 2^m$ P.M. the westerly movement was $5''\cdot45$; after this the movement was more gradual, the boom finally going out of range at 2.20 A.M. after a total westerly movement of $10''\cdot9$.

On December 6 and 7 it is impossible to estimate, even roughly, how the movements continued, as there was not sufficient play to re-adjust the instrument with the level screw, and it was necessary to employ the coarse screw, which carries no graduations.

On December 5, to the east of the instrument, the weather was fine and the Sun shining brightly; we have reason to suppose, therefore, that the extraction of the moisture by the action of the Sun's rays would cause the Earth to contract and consequently to sink, while to the west of and at the place of observation heavy rain was falling on the dry Earth, which caused it to swell and consequently to rise relatively to the ground east of the instrument.

On December 6, under nearly the same conditions, the instrument behaved in nearly the same manner. On December 7 the instrument tilted so gradually as to indicate that the greater part of the westerly movement was caused by the gradual settling-down of the western side after the upheavals of the two previous days.

From the behaviour of the boom on these days it may be suggested that the diurnal variation of level at Mauritius is due to the difference in the quality of the soil and vegetation on the east and west sides of the instrument; but if it should be found that in all parts of the world there is a relative upheaval to the west of the instrument from a little after sunrise till the afternoon, followed by a corresponding depression till morning, then it seems evident that, in general, there is a land-tide—a depression—caused by the extraction of moisture from that portion of the Earth nearest the Sun, and propagated round it from east to west. This is, perhaps, a bold statement to make, but the enormous solar effects observed in Mauritius have led me to the conclusion that such a thing is possible, though by no means as yet proven. T. F. CLAXTON.

Mauritius, 1899 March.

*Early Meteorology.**

IN a recently published volume the Berlin Professor of Meteorology has reproduced twenty-three rare 15th and 16th century publications dealing with weather prognostications or with accounts of exceptional meteorological phenomena. In the introduction we are told that the Greeks from the times of Homer and Hesiod down to those of Aratus were guided by so-called natural indications—winds, clouds, luminous phenomena, &c. About the time of Meton a kind of weather calendar, giving the probable seasonable weather in accordance with general experience, began to be displayed on columns for public guidance, together with the predicted risings and settings of the stars, eclipses, and other purely celestial phenomena. For a full account of these tablets or parapegms we are referred to a paper by Prof. G. V. Schiaparelli, in the 'Annuario meteorologico italiano' for 1892. It seems very probable that the juxtaposition of these two classes of information gave rise to, or at least tended to foster, the popular belief that the actual daily weather was a direct result of the relative positions of the heavenly bodies. Later on this notion was energetically combated by Geminus, only to be more generally developed by Claudius Ptolemy in the second century of the Christian era. Through the labours of the Arabs the astronomy, astrology, and meteorology of the Alexandrian school were introduced into Europe by way of Spain towards the end of the twelfth century, and about this period the first endeavours were made to predict not only the weather but also political and religious changes for each coming year, in the same way as had long been practised in regard to the chief astronomical phenomena. Thus were produced the various smaller manuscripts which are still found in our libraries with such titles as "Iudicium anni...," "Vaticinium anni...," or "Prognosticon anni..." Shortly after 1470 these prognostications began to appear in print, at first in Latin, but eventually in the vernacular languages. They appeared in many lands, but to Germany belongs the dubious fame of having produced the greatest number—probably not less than 750—of these ephemeral publications. The printing of these Prognostications was not without its good influence, as many of their authors began to compare the predicted weather of the calendars with that which actually occurred. In this way many records of extraordinary weather phenomena have been handed down to us, although not a single complete copy of one of these Weather-journals of the 16th century seems to have survived. The editor has not restricted himself to German specimens, for although there are sixteen of these, France is represented by three, Italy also by three (two of which describe great floods at Rome 1495 and in 1530), while Spain, England, Holland, and Denmark furnish one each, although the Danish pamphlet—printed at Uraniburg—is in Latin. Excepting a couple of reprints of minor importance, all the pamphlets and broadsides are most faithful reproductions of the originals, not only as regards the antique letterpress and illustrations, but also in the discolorations and accidental defects of the paper. The earliest document is a German broadside, "Praktik," for A.D. 1486, with prognostications of the weather for certain days of the year, often with reference to the phases of the Moon. The general remarks are almost altogether of an astrological character, and it winds up with a diagram of the aspects of the astrological houses for the vernal equinox. This is followed by a representative series of "Practicas" and "Prognostications" for the closing years of the 15th century and the beginning of the 16th, filled for the most part with vague astrological vaticinations. An examination shows that the few precise astronomical data are invariably copied from Regiomontanus, whose calendar for the years A.D. 1475 to 1531 was for more than half a century the only reliable almanac on which navigators and geographers of this transcendentally important period could depend*. Of much interest is the reproduction of a rare German broadside, "Bauernkalender," or farmer's almanac for A.D. 1548. In these almanacs, which were meant for the illiterate, the days of the month were usually marked by triangles—black for weekdays and red for Sundays. In addition to the time-honoured signs for the four quarters of the lunation, many other symbols were used—some to denote the predicted weather, some to mark the days favourable for sowing or planting, and some even those for bathing or blood-letting. The trefoil, we find, indicates the days suited for sowing; a rude figure of a glove denotes cold weather, while a broad-brimmed hat marks the days of bright sunshine. Each festival, and each day dedicated to a saint or martyr, has its own tiny illustration, thus:—Epiphany is recognised by the figures of the three Magi bearing gifts, or in the calendar before us by three crowns surmounted by a star. On Shrove-Tuesday we have a masked face decorated with the cap and bells, and so on for the rest. The eclipse of the Moon, which occurred at Augsburg on April 22 at 11 P.M., is indicated by a partially obscured figure of the Moon, with what seem to be the signs for the correct hour placed to the *right* to show that the obscuration occurred after mid-day*. The study of this old calendar is much facilitated by a comparison with the tiny and very curious Bauernkalender for the present year, which the editor has thoughtfully added to the volume. These curious survivals from former times, we are told, still have a large circulation in the remoter districts of Austria.

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* Neudrucke von Schriften und Karten über Meteorologie und Erdmagnetismus herausgegeben von Prof. Dr. G. Hellmann.—No. 12. Wetterprognosen und Wetterberichte des XV. und XVI. Jahrhunderts. 4°. Berlin: A. Asher & Co., 1899.

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* A comparison of the eclipses of the Moon in this wonderful almanac with v. Oppolzer's 'Canon der Finsternisse' shows that, omitting two eclipses into which some error of computation has evidently crept, the mean error for the remaining 39 predictions is but 16 minutes of time. On the whole the predicted times are too early by some eight minutes. It was doubtless from this source that Columbus derived his knowledge regarding the eclipse of the Moon on 29 February, 1504, which he so opportunely turned to account in his dealings with the inhabitants of Jamaica.

Another of the broadsides places before us in *facsimile* "An Almanacke and Prognostication made for the yeare of Our Lorde God, M.V.C.LV made by Maister Anthony Askham Phisician and Preste," from the British Museum. Here, too, the astrological element comes in, and we find that when the Moon is in such and such signs we may "take preparatiues or disgestion," or when she is in . . . Aquarius or Pysces we may "cut heyres," with many other precepts of the kind. Weather predictions are given for the quarters of the Moon in a style which, in one instance at least, is delightfully anticipative of the 'Yellowplush Papers,' for in September we read "The fyrst quarter the xxiiii. day at viii. of the clocke at nyghte many hayle showres and cold a day or .ii." In 1555 there was evidently the same difficulty that still exists with regard to the civil and the astronomical date, since at the foot of the sheet we find "The Eclypse of the moone shalbe iiii. daye of June at .ii. after midnyght & shalbe all dark & continue ,ii, houres & ,ii, minutes, & maruelous to beholde," while in the column for June we read "The full Moone Eclipsall the .v. day at .ii. of the clocke before none cloudy wyth sum shoures." In the 'Canon der Finsternisse,' in which Greenwich mean civil time is employed as "Weltzeit," the middle of the eclipse is set down as occurring at $2^h\ 33^m$ on June 5, duration $3^h\ 44^m$, totality lasting $1^h\ 44^m$.

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* I take this opportunity of correcting an oversight in the *Catalogue of the Crawford Library of the Royal Observatory, Edinburgh.* On page 73, in the note to Bawren Kalender 1540, *for* March 8, *read* April 7 (civil date).

The four last broadsides call for special mention. The first represents in most naive and graphic style a display of mocksuns witnessed near Munich in the year 1509. The next gives an obviously carefully executed figure of a complicated system of mocksuns and luminous arches seen at Wittenberg in 1551. The description is in Latin. The third depicts the threatening appearance of what was evidently the cloudy axis of a small tornado which hung over the city of Augsburg shortly after noon on 2 July, 1587. It is described as swaying now to the right, now to the left, and finally forming a sharp point at the lower end. The volume closes with the reproduction of a large engraving of a magnificent aurora which covered the whole sky with converging luminous arches at Augsburg on 17 November, 1605. The rays are described as having been partly red like fire, and partly white, and as shooting towards each other not unlike lightning, although not so fiery. Scientists and booklovers will find very much to interest them in this most desirable volume.

R. Copeland.

Comparison of the Number of Stars in the Greenwich Astrographic Catalogue, in the Bonn Durchmusterung, and in the Astronomische Gesellschaft Catalogues, Declination +64° *to* +70°.

The plates for the Greenwich Astrographic Catalogue have now been measured from declination +64° to +70°, and the number of stars in the Catalogue for that portion of the sky has been provisionally determined. The following table shows the distribution of stars for each hour of right ascension in the Astrographic Catalogue as compared with the Bonn Durchmusterung and the Astronomische Gesellschaft Catalogues (Helsingfors and Christiania), together with the number per square degree and the ratios to the B.D. and A.G.C. The numbers given for the Astrographic Catalogue are subject to revision when the Catalogue is actually printed, but it is not probable that they will be materially altered. The numbers given represent the actual number of separate stars, each star being, as a general rule, shown on two plates which were measured at the same time in the duplex micrometer. The method of measurement adopted avoided any duplication of stars in the Catalogue from overlap of the plates, an important point in discussing the distribution of stars in the sky. Exposures of 6^m and 3^m were given to all the plates, with a supplementary exposure of 20^s. All plates which failed to show stars of 9·0 mag. of Argelander's scale with the 20^s exposure were rejected.

R.A. h	No. of stars measured on plates.	No. per square degree.	No. of stars in B.D.	Ratio.	No. of stars in A.G.C.	Ratio.
0 ..	2283	64·9	596	3·8	266	8·6
1 ..	2317	65·9	601	3·9	244	9·5
2 ..	1921	54·7	432	4·5	199	9·7
3 ..	1948	55·4	371	5·3	173	11·3
4 ..	1959	55·7	415	4·7	192	10·2
5 ..	2821	80·3	331	8·5	185	15·3
6 ..	2561	72·9	291	8·8	181	14·2
7 ..	1978	56·3	309	6·4	178	11·1
8 ..	2025	57·6	338	6·1	169	12·0
9 ..	1373	39·1	301	4·6	151	9·1
10 ..	1623	46·2	300	5·4	164	9·9
11 ..	1845	52·5	264	7·0	168	11·0
12 ..	1631	46·4	261	6·3	138	11·9
13 ..	1726	49·1	295	5·8	159	10·9
14 ..	1763	50·2	318	5·6	186	9·5
15 ..	2160	61·4	328	6·6	200	10·8
16 ..	2116	60·2	345	6·1	207	10·2
17 ..	3093	88·0	422	7·3	234	13·3
18 ..	3242	92·2	425	7·6	211	15·5
19 ..	3636	103·4	538	6·8	246	14·8
20 ..	2663	75·8	478	5·6	191	14·0
21 ..	4446	126·5	703	6·3	329	13·5
22 ..	4217	120·0	654	6·4	275	15·4
23 ..	2829	80·5	663	4·4	320	8·8
	58170	70·0	9971	5·83	4966	11·7

W. H. M. Christie.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Visitation of the Royal Observatory.

Gentlemen,—

An expression in an article of the 'Standard' on Monday last on the Visitation of the Royal Observatory is a striking instance of how little is known of the early history of the institution of which we are so justly proud, and which, though not the oldest of its kind, has a longer record of steady and persevering work than any other. The expression to which I allude is that the visitation of the Observatory was instituted by Flamsteed in 1712. It was in December 1710 that a royal order in council was obtained, at the instance of Newton, that the Royal Society should have power to visit and inspect the Observatory, the principal object being that the results of the observations should be all made sooner available for theoretical deductions than Flamsteed was disposed to make them. This order greatly excited the wrath and anger of the latter; but I have no wish to dwell at length upon the most painful episode that ever occurred in the history of our science. Flamsteed often compared his wrongs to those of the noble Tycho, and the exhaustive account of the latter published a few years ago by Dr. Dreyer shows that the fault in that case also was partly on his own side, though in a very different way. The real moral of the dispute between Newton and Flamsteed seems to be the unfortunate circumstance that the Government which founded the Observatory took absolutely no steps to provide it with the means of making observations, to say nothing of allowing the astronomer (M.R., or Mathematicus Regius, as Flamsteed was fondest of calling himself) a miserably small stipend, taking advantage of the fact that he was in holy orders to make partly up for this by putting him (several years afterwards) into a living the patronage of which was at the disposal of the crown. Nothing appears to have been done in consequence of this order until the end of October 1711, when Flamsteed was summoned to meet the President and other members of the Board at the Royal Society's rooms in Crane Court. It was on this occasion that, after he had stoutly maintained that the instruments were his own property and he would allow no one else to interfere with them, Newton remarked that it would be "as good to have no observatory as no instruments." This truism again enraged Flamsteed, and although the wording of the dictum would have been more logical had the Government provided the instruments and Flamsteed the observatory, Newton's meaning evidently was that the provision of the building by the former must be allowed to give it some rights in the matter.

But, as I said before, the subject is a painful one to all lovers of astronomy, and I will not farther dwell upon it. What I wish to point out is that the royal order in question lapsed at the death of Queen Anne in August 1714, and nothing further seems to have been done till early in the reign of George III. Soon after the death of Bliss (who succeeded Bradley as Astronomer Royal in 1762 and died in 1764), and before the appointment of his successor, a memorial was presented to the king by Lord Morton, then recently elected President of the Royal Society, suggesting that steps should be taken and Visitors appointed to secure the regular and continuous course of observations at the Royal Observatory, the careful custody of the results without power of removal, besides some other matters of order, such as the prohibition of the indiscriminate admission of strangers. This was acceded to, and by royal warrant, on 1765 February 22, the President of the Royal Society and such other members as its Council should appoint for the purpose, were constituted permanent Visitors of the Observatory, with power to inspect the instruments and receive regularly copies of the observations made with them. Soon after this Maskelyne was appointed Astronomer Royal. Two years later, in 1767, at the instance again of the Royal Society, an order was obtained for the regular printing of the observations and the payment of the expense by the Government. The order for this appointment of Visitors, lapsing at the death of George III., was renewed by George IV. on 1820, May 19, at the very time (as I pointed out in my letter last month) that Baily circulated that suggestion for the establishment of an Astronomical Society which was practically taken up in the following year. When the next king, William IV., ascended the throne in 1830, the former warrant having become void, a new one was issued, by which to the President and six Fellows of the Royal Society, other members were added to the Board of Visitors, consisting of the President and six Fellows of the Astronomical Society, together with the Professors of Astronomy at Cambridge and the Savilian Professor at Oxford, ex-Presidents remaining members. This arrangement was renewed, we may presume, at the accession of her present Majesty, and the Board, as the Astronomer Royal's Reports have lately shown, is still constituted as above. I need hardly add here that Airy commenced at his appointment the custom of presenting a printed Report to the Board of Visitors, giving a succinct account of the work effected during the previous year; it is enough to compare the size of his early reports with the later, and still more with those of the present Astronomer Royal, to see how greatly this work has increased in the last sixty years.

Yours faithfully,

Blackheath, 1899, June 6. W. T. LYNN.

The Gegenschein.

GENTLEMEN,—

In reference to Professor Barnard's article on the Gegenschein, it may be worth noting (though the point is rather physical than astronomical) that the glow around the shadow of his head, which the Professor has observed when the Sun is low, is due, not so much to the absence of shadows from the blades of grass at the point opposite to the Sun, as to direct reflection from their surfaces, aided by contrast with the dark shadow of the head. Careful observation will prove that this is so, and it is shown by the great increase of the effect when the grass is covered with dew, the globules of which are, of course, highly reflective. The appearance is really striking when first seen, as it may be by anyone, though it is surprising how few people seem to have noticed it.

Benvenuto Cellini first observed it when in his 40th year, as he tells us in his Memoirs, where he says, after relating various wonderful things that happened to him:—"I will not omit to relate another circumstance also, which is, perhaps, the most remarkable which has ever happened to anyone," and which came to him, he says, as the direct gift of God. "Ever since the time of my strange vision" (previously narrated) "until now" (when he is nearly sixty years of age), "an aureole of glory, marvellous to relate, has rested on my head. This is visible to every sort of man to whom I have chosen to point it out; but those have been very few." (One wonders they did not see it round their own heads also.) "This halo can be observed above my shadow in the morning from the rising of the Sun for about two hours, and far better *when the grass is drenched with dew*. It is also visible at evening about sunset.

"I became aware of it in France, at Paris, for the air in those parts is so much freer from mist, that one can see it there far better than in Italy, mists being far more frequent among us. However, I am always able to see it and to show it to others; but not so well as in the country I have mentioned." (Cellini's Life, Symonds's Translation, vol. ii. p. 111.)

Branch Hill Lodge,
Hampstead Heath, N.W.
1899, June 15.

Yours faithfully,
B. WOODD SMITH.

The Parallax of the Gegenschein.

GENTLEMEN,—

Prof. Barnard in his very interesting article on the Gegenschein, which is abstracted in the June number of the 'Observatory,' mentions my theory, which assumes it to be a tail to the Earth, produced by the escape of molecules of helium and hydrogen away from the Earth in a direction opposite the Sun. He remarks, however, that the absence of any parallax is fatal to this theory.

Perhaps the following considerations will show that the absence of any observable parallax does not invalidate the tail theory.

First, I assume that our tail is a very long one, that is, of the order of millions or tens of millions of miles. This assumption is justified by the analogy of comets' tails.

Secondly, that the gases composing the tail are in a state of extreme tenuity, and the light which we see is the integrated result of the excessively feeble reflexions coming from all distances along the axis of the tail, *excepting* those nearer parts where it will be hidden in the Earth's shadow.

Now it is only the nearer parts, say within the Moon's distance, which could show a horizontal parallax exceeding one degree, and an angular displacement less than this would be impossible to observe with any certainty in so ill-defined and faint an object.

Judging from the large apparent size of the gegenschein, it would seem unlikely, however, that the tail would be wholly hidden within the Earth's shadow between us and the Moon's distance; there would be outlying parts which, it might be thought, should give evidence of parallax.

But would these outlying nearer parts be visible at all? Suppose that the tail were presented to us sideways instead of end on, I believe that it would be absolutely invisible, even on the darkest sky. According to my view it is only on account of the enormous depth of excessively rare gas through which the line of light passes that we are able to perceive anything whatever. For this reason I think that the nearer portion of the tail, lying outside the central parts obscured by the Earth's shadow, would not reflect enough light to be separately visible.

Admitting that the gegenschein *is* an extra terrestrial phenomenon, and that it *is* due to reflected sunlight, the absence of any indication of a dark centre, such as would be caused by the shadow of the Earth projected upon it, shows that the light, or a large proportion of it, must come from a region beyond the apex of the shadow-cone, so that, so far as I can see, it comes to this: either we must assume that there exists at an immense distance from the Earth, yet always in the same relative position to it, a round disk or roughly spherical mass of matter (dust-like or gaseous); or that there is an elongated stream of matter, the axis of which is directed towards the Earth.

The latter seems to me to be the least difficult to imagine, because the outward flow of matter along the stream, as in a comet's tail, explains the constant relative position.

The weakest point in my theory, I think, is this:—If the tail spreads out in the form of a cone say of 15 degrees, the intensity of the reflected light ought to diminish very rapidly with the distance from the Earth, and a large proportion of the total illumination as seen from the Earth should in this case come from the nearer parts of greater density. Consequently there should be evidence of the Earth's shadow seen as a dark space in the centre and evidence of parallax in the outer ring of light.

With regard to Mr. Anderson's objection, of course a more satisfactory theory would be one which explained both the gegenschein and the zodiacal light. I do not see, however, why the mere optical superposition of the gegenschein upon the zodiacal band should show that there is any real relation between them. Nor do I see why it should be considered to be "obviously connected" with the zodiacal band.

Prof. Barnard states, in the article referred to above, that the zodiacal band is not always present. No distinct band is visible in August and September or in March, so that at these times the gegenschein is just as obviously an independent phenomenon.

Yours faithfully,
J. EVERSHED.

Kenley, 1899, June 4.

Cyclonic Motion in a Sun-spot.

GENTLEMEN,—

I was much interested in the account in the April number of the 'Observatory' of cyclonic motion among a group of sun-spots that appeared on the Sun in the middle of March. Cloudy weather here allowed few observations of that group, but when it had nearly completed its transit, on March 26, a spot appeared inside the East limb in which a tendency to cyclonic motion was also noticeable. As there may be much significance in the details of spot-formation during periods of minima, I beg leave to enclose a record of the observations of thirteen consecutive days, and some drawings of the spot, as seen with a Brashear telescope four inches in aperture and a solar prism *. As the solar axis inclines from the Earth in the beginning of March, the path of a spot assumes a seeming convexity towards the north pole of the Sun, which creates a slight vortical effect to the amount of about 14½ degrees; but the motion of the umbra of this spot seemed to be 45 degrees more than that attributable to mere perspective. It moved in the direction of the hands of a watch, and being in the southern hemisphere of the Sun accorded thus with M. Faye's theory of spot-formation.

I hope to hear further information from those who have facilities for accurate measurement. Having observed the Sun's surface telescopically for more than eight years, I am much interested in all the details of the approaching minimum.

I am, Gentlemen,
Very truly yours,
ROSE O'HALLORAN.

1427 Post Street, San Francisco,
1899, April 21.

A Statesman's Astronomy.

GENTLEMEN,—

The recently published 'Notes from a Diary' by Sir M. E. Grant Duff contains numerous passages of an astronomical import suggested to the writer by his residence in India for five years as Governor of Madras. The following, being one of such passages, may find a place in the 'Observatory':—

1884, March 25. "As last year, the stars are a great pleasure. When we walk on the Terrace after dinner, Orion is very glorious. So is the Great Bear. Jupiter and Mars are just overhead. When I look from the verandah of my rooms, now in the garden-block of the house, a little after ten, the Southern Cross is rising over the solitary Casuarina; the False Cross is straight opposite me and higher; Canopus is nearer to the garden. Late in the night, when

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* [Miss O'Halloran has sent us with this letter a daily record of observations of a sun-spot in heliographic latitude 12° S., and drawings of the same, which we regret we have not space to publish.—EDS.]

I return, as I often do, to the verandah, the Scorpion has become a splendid object in the South-east, and the Southern Cross is standing nearly erect, where the False Cross lay inclined." (Vol. i. p. 263.)

What his Excellency means by the "False Cross" I do not know: the designation is new to me.

Northfield Observatory,
Eastbourne,
1899, June 10.

Your obedient Servant,
G. F. Chambers.

Confirmed or New Variable Stars.

Gentlemen,—

I am able to confirm or report the variability of the following orange stars, although they do not appear in Chandler's 'Third Catalogue of Variable Stars':—

1. Star.	2. Bm.	3. Constellation.	4. R.A. 1890.	5. Dec. 1890.	6. Mag.	7. Average colour.	8. Spectrum type.
			h m s	° ′			
B.D. +50°·961	...	Perseus.	4 8 18	+50 20	9·5		
Es.-Bm. 664 ...	554	Cygnus.	20 17 13	+36 35	9·5	YOr$^{0\cdot5}$*	IV.
665 ...	...	,,	20 17 33	+37 11	9·4	ROr$^{4\cdot2}$*	IV.
666 ...	...	,,	20 17 40	+40 6	8·8	Or$^{3\cdot4}$*	III.
728 ...	610	Cepheus.	22 18 59	+55 24	7·2	Or$^{3\cdot6}$	IV.
764 ...	658	Cassiopœia.	23 55 39	+59 45	7·8	Or$^{4\cdot3}$	III. ?

Col. 1 gives the designation of star, Es.-Bm. denoting Espin's Revision of Birmingham's Catalogue; col. 2 gives the number in Birmingham's Catalogue of Red Stars; col. 6 gives the B.D. magnitude; col. 7, the average colour from my observations, the notation being the same as that in the letter on p. 278, July 1897 number of 'The Observatory'; where marked with an asterisk the stars were too faint to tell the colours except roughly; col. 8 gives the type of spectrum as given by Krüger in his 'Catalog der Farbigen Sterne.'

B.D. +50°·961 was announced by Espin as deep orange. It is too faint to judge this with my telescope. Its variation appears to be irregular; its range exceeds ½ mag.

Bm. 554. Variation slight, perhaps not ½ a mag. Period very long or irregular.

Es.-Bm. 665. Variation slight, perhaps under ½ a mag. Period long or irregular.

Bm. 554 and Es.-Bm. 665 are suspected of variability by Espin.

Es.-Bm. 666. The variation of this star was announced by Espin. So far as I have observed it, the variation is under ½ mag. Period probably irregular. Its colour was observed by me as midway between $\mathrm{Or}^{3\cdot 4}$ and $\mathrm{ROr}^{3\cdot 4}$, but the star was too faint to tell very exactly.

Bm. 610 is in Gore's List of suspected Variables. The variation is slight, perhaps under ½ a mag.; period irregular.

Bm. 658. Variation announced by Secchi and others. So far as I have observed, it amounts to about ½ mag., but I have only observed it a short while.

The variation of some of these stars being slight, the question arises, How much variation is necessary to constitute a variable star? Professor Pickering, in examining into the stars in clusters, draws the line at ½ mag. If so, probably some long recognized variables should be struck out of the catalogues; and what title should be given to those whose variation is less than half a magnitude? The expression "unsteady" is sometimes used with regard to such. Has not the time arrived for an extended classification of variable stars? The five classes proposed by Professor Pickering many years ago may do very well, as far as they go; but should they not be subdivided, in the same way as the spectral classes are? Probably no one is more competent to do this than Mr. S. C. Chandler, and when he next brings out an edition of his catalogue, a column for the class would be helpful.

West Hendon House, Sunderland,
1899, June 12.

Yours faithfully,
T. W. Backhouse.

OBSERVATORIES.

Royal Observatory, Greenwich.—The Annual Visitation took place on June 3, when the Board of Visitors, under the presidency of Prof. G. H. Darwin in place of Lord Lister, who did not attend, received the Astronomer Royal's Report for the period between 1898 May 11 and 1899 May 10. Extracts from the Report are given below. The general company included several distinguished strangers — Profs. Newcomb, Michelson, Barker, and Langley from across the Atlantic, Prof. Cornu, of Paris, and others.

Buildings.—The new Observatory Building, which has been in progress since 1891, is now completed. It seems necessary, for the proper display of this handsome structure, that the boundary of the Observatory should be enlarged, and a proposal to give effect to this is under the consideration of the Admiralty. A new Accumulator House, necessary for the extension of the electric lighting to the east and west wings, has been made as a basement on the north-west side of the new building. The new Magnetic Pavilion, where the instruments for determination of absolute magnetic elements have been deposited, has been erected in the Park at a distance of 350 yards east of the Observatory. A balcony has been mounted around the building, which contains the 28-inch telescope.

Transit Circle.—The number of observations of transits made was 11,764, of zenith-distance 10,830. The latter number includes 665 observations of the nadir, and 560 observations of stars by reflection. The number of stars observed in 1898 is about 5000. The variation of the circle-reading of the nadir-point depending on the hour of the day, mentioned in previous Reports, appears still to exist (see Mon. Not. R. A. S. vol. lix. pp. 351–353), and the value of the R – D discordance remains small, as in 1897. The co-latitude of the transit-circle found from observations of about 600 stars in 1898 was $0''\cdot15$ less than its adopted value, and it appears from an investigation that this is a true mean value and is not affected by any real variation of latitude which may exist.

The mean error of the Moon's tabular place (computed from Hansen's lunar tables with Newcomb's corrections) is $-0^{s}\cdot143$ in R.A., and $+0''\cdot14$ in N.P.D., deduced from 104 observations. The copy for press of the new Ten-Year Catalogue of Stars, 1887–96, which contains 6,950 stars, is nearly completed.

The New Altazimuth.—Modifications have been made in the arrangements for counterpoising the instrument. The essential part of the final arrangement, which appears to be successful, consists of ball-bearings, with balls of large diameter in a hardened steel ring, freely suspended by a chain. Discordances in the results of observations in reversed positions of the telescope as compared with the transit-circle observations indicated weakness in the attachment of the telescope-tube to the axis of the object-glass and of the eye-end to the telescope-tube. These possible weaknesses were remedied, and the object-glass re-centred and re-fixed. The observations in reversed positions now show a satisfactory accordance.

Equatorials.—A new 30-inch mirror made by Dr. Common has been attached to the Thompson Equatorial and with it a series of photographs of the planet Eros has been taken, as well as 9 photographs of Neptune and its satellite, and others of comets, the Andromeda Nebula, and the Moon. With the 26-inch refractor 12 successful photographs of Neptune and satellite have been taken with an occulting shutter to screen the planet during the greater part of the long exposure necessary to show the satellite; 42 successful photographs of double stars have been obtained, for some of which the occulting shutter was used. The 28-inch refractor has continued to be used for the observation of double stars, the results of which have recently been communicated to the Royal Astronomical Society. The Sheepshanks Equatorial has been used, as heretofore, for observations of occultations of stars by the Moon and for observations of position of comets.

Of the 1149 fields which make up the portion of the Astrographic Chart allotted to Greenwich, there remain 122 to be photographed for the chart and 119 for the catalogue. The number of plates measured in the twelve months was 144, each in two positions, direct and reversed. The measurement is now complete from Dec. 64° N. to 70° N. and for rather more than half the zone of the next degree of declination. The printing of these completed measures will be soon begun.

Spectroscope.—The observations with the reflector above recorded have delayed the work with the new photographic spectroscope. The adjustment of this has now been taken in hand.

Solar Photography.—The use of the Dallmeyer photo-heliograph was discontinued in 1898 July, since which time photographs have been taken with the Thompson photo-heliograph of 9-inch aperture. The record is complete for 357 days of the 365, photographs having been taken at Greenwich on 165 days. The chief incident in this department during the year was the temporary revival of activity from the end of July until the middle of November, which culminated in the great spot of September.

Magnetism and Meteorology.—The determination of variations of terrestrial magnetism have been continued in the same place and with the same instruments as in former years. The absolute determinations are now made in the new pavilion. Observations of declination are still made also at the old site, to give corrections to the observations previously made in that place. The meteorological reductions for the year are in a forward state. The mean temperature for the year 1898 was $51^{\circ}\!\cdot 3$, being $1^{\circ}\!\cdot 8$ above the average for the 50 years 1841–90.

The only extraneous work during the year was the determination of the difference of longitude between Greenwich and Killorglin, effected by Mr. Dyson and Mr. Hollis in October and November last. Killorglin is at the head of Dingle Bay, Ireland, and its longitude was determined to supplement previous determinations of the longitude of Valentia and Waterville. The station was chosen to eliminate as far as possible the effect of local attraction due to the mountains in the locality. At the suggestion of the International Geodetic Association, a re-determination of the longitude Paris – Greenwich is contemplated.

The arrangement of the staff remains as in previous years.

UNIVERSITY OBSERVATORY, OXFORD.—The astronomical work reported by the Savilian Professor to his Board of Visitors as having been done during the past year is mainly the continuation of the formation of the Astrographic Catalogue. The number of plates allotted to this Observatory is 1180, and of these 586 are measured and 525 completely reduced. The corresponding numbers last year were 448 and 255 respectively. This completes the third year of the work. The Royal Society have granted £150 per annum hitherto, and have promised the same sum for a fourth year. Prof. Turner originally estimated that the whole work would be done in five years, but now has to revise his estimate and anticipates that possibly another year will be required. Having in mind the result of an investigation recently communicated to the Royal Astronomical Society, from which it appears that plates two degrees square are practically free from distortion effects, Prof. Turner thinks it a waste of time to take chart-plates of the regulation size, and does not propose to take the long-exposure plates. Some interesting statistics are given in the Report. On one plate, R.A. $19^h\ 34^m$, Dec. $+29°$, 1996 stars were measured, and on the next in the zone (R.A. of centre $19^h\ 43^m$) 2440 stars. On both plates between 300 and 400 stars are visible taken with 20 sec. exposure.

The instruments are in good order. The Barclay Transit-circle has shown some changes of collimation, which have been traced to looseness of the object-glass in its cell. This having been remedied, the sudden changes have entirely disappeared. The large dome requires re-covering, and it is suggested that this should be done with canvas, as *papier-mâché* would be considerably more expensive.

The need of a residence at the Observatory for the Director is not less necessary than in former years.

BIDSTON OBSERVATORY, LIVERPOOL.—From the Report of the Director for the year ending 1898 December 31, we gather that, beyond the usual meteorological observations, Mr. Plummer has made 2535 observations with the transit, and has used the equatorial for observing comets, planetary diameters, and double stars. The results of the cometary work are published in the 'Monthly Notices,' but the diameter and double-star measures do not appear to have been published. The number of chronometers tested during the year was 202.

PUBLICATIONS.

POTSDAM MEASURES OF ASTROGRAPHIC CHART PHOTOGRAPHS.—The Potsdam Observatory is, so far as we are aware, the first to publish any of its measures of the photographs taken in connection with the International Chart of the Heavens. The Potsdam zone extends from N. Dec. 31° to 40°, and comprises 2636 square degrees out of the 40,000 of the sky; the number of photographs in the zone being 1232. The volume published contains the measures of 57 plates, and gives the apparent rectangular coordinates—*i. e.*, the mésures bruits—of 20,627 stars.

The introduction to the volume contains a short description of the instrument and of the measuring-apparatus. This is followed by a determination of the probable error of the measures. Comparison of the rectangular coordinates of the same stars on two plates gives an independent and satisfactory method of obtaining this, and from 150 stars it is found that the probable error is $\pm 0''\!\cdot\!193$ in the x coordinate and $\pm 0''\!\cdot\!171$ in the y coordinate. The personal equations of the measures are briefly discussed; the somewhat large value of the personal equation for the y coordinate of one of the measurers, Miss Everett, varying from $-0''\!\cdot\!64$ for stars of the 6th magnitude to $0''\!\cdot\!00$ for stars of the 11th, would seem to show the desirability of measuring each plate in reversed positions, so as to eliminate this quantity.

For the determination of the magnitudes of the stars, it was assumed that the faintest objects visible on the plates were of magnitude 11·0; the average 9th magnitude was determined by comparison with the 'Bonn Durchmusterung,' and intermediate magnitudes were interpolated. Dr. Scheiner considers that the limits of magnitude of the faintest stars on the Potsdam plates do not vary more than from $10^{m}\!\cdot\!5$ to $11^{m}\!\cdot\!2$.

The number of stars shown on a plate varies from 38 to 1830, and the proportion to the number given in the B.D. for corresponding areas varies from 1·0 to 11·7. Dr. Scheiner concludes that this ratio varies with the density of the stars in the sky. He concludes that there is a want of homogeneity in the magnitudes of the 'Bonn Durchmusterung' which he considers estimates about half a magnitude lower for the stars from $9^{m}\!\cdot\!0$ to $9^{m}\!\cdot\!5$ for the stars in the poor regions than it does in the Milky Way. It may be questioned whether sufficient allowance is made for the variation of the plates arising from the quality of the night; the figures given on p. 269, obtained from the Greenwich photographs, which are deduced from 400 plates, and in which the inequality of the plates is largely eliminated, confirm the result that in the rich regions of the sky the proportion of stars on the photographs to the number in the 'Bonn Durchmusterung' is much greater than in the poorer region.

With the measured rectangular coordinates are given the Right Ascension and Declination obtained from these measures to 1^{s} and $0'\!\cdot\!1$ respectively, the estimated magnitude, and the B.D. number and magnitude of stars contained in that work. Lists are also given of stars in the B.D. which are missing on the plates. The comparison with the B.D. is of undoubted advantage.

The approximate R.A. and Decl. are useful only for identification, and as these are easily obtained for any star from the rectangular coordinates (requiring multiplication by 24 approx. for the R.A. and 5 for the Declination), it seems a pity to have taken up space by printing them. Judging from this volume 20 similar volumes will be required for the whole zone, and if other observatories are equally lavish, the Astrographic Chart of the heavens will be a somewhat bulky work. Another criticism we should make is that the plates measured are not consecutive in a zone, but we are glad to see the first instalment of the Astrographic Chart, and hope to see a good deal more of it before long.

F. W. Dyson.

MEMOIRS OF THE ROYAL ASTRONOMICAL SOCIETY, VOL. LIII.—This recently-published volume contains the following papers:—"Measures of Double Stars," by W. Coleman; "Theory of Lunar Motion," by Ernest Brown; "Determination of Terrestrial Longitudes by Photography," by Capt. Hills; "Division Errors of the Greenwich Transit Circle," by F. W. Dyson and W. G. Thackeray; "Theory of Lunar Motion," by Ernest Brown; "Radiant Meteor Showers," by Denning; "Measures of Double Stars," by W. Maw. The papers are published in the order given, and it looks strange to see the volume beginning with one set of double-star measures and ending with another set, and to find two other papers wedged in between the two sections of Mr. Brown's paper. No doubt the point has been considered; and it seems unavoidable if the present system of paging is adhered to. The papers are necessarily printed in the order received, and must be paged accordingly; it would not be feasible to wait until the last paper was received before beginning the printing. Might we suggest that the paging should be complete for each paper, so as to permit of some arrangement in the binding? Mr. Coleman's measures were made with an 8-inch Cooke refractor. There are 175 pairs, in 109 of which the components are separated less than 5″, and in 66 pairs the separation is over 5″. The measures are welcome by reason of the fact that many of the stars have not been measured since Dembowski's time. There are a few printer's errors, all fairly obvious, so that no one will be misled by them. The measure of Σ 117 in 1896 should be 26″·78, that of Σ 2272 should be 1895·607 not 1805·67; the minutes are left out in the declination of Σ 2276; Σ 1172 should be ι Boötis; and Σ 2161 should be ρ Herculis. Both in this paper and in Mr. Maw's the wrong Struve number is applied to η Cassiopeiæ (Σ 60). But these are scarcely blemishes on an excellent set of measures, on which Mr. Coleman is to be congratulated. We need make no remarks on Mr. Maw's measures for the years 1895–98. They form a valuable continuation of his former work. Mr. Maw has now added an 8-inch to his equipment. In Mr. Coleman's paper the measured distance is given first, and then the angle, while Mr. Maw reverses this order. This difference of arrangement is a source of unnecessary trouble to the reader when referring to the two papers in turn.

PUBLICATION No. 14 of the Cincinnati Observatory, which is described in its sub-title as "A Catalogue of 2030 Stars for the Epoch 1895," will probably be better known as Mr. F. G. Porter's "Second" Catalogue of Proper Motions. Since the erection of a new Meridian Instrument in 1888 Mr. Porter has himself reobserved some 4000 stars, and redetermined the proper motions of over 2000 of these stars by the method of least squares. Those who are conversant with this sort of work will be able to appreciate the amount of labour involved in hunting up observations from old catalogues, and in reducing them to a single epoch upon a uniform system of reduction. The places of the fundamental stars used for obtaining the clock-error and equator-point have been taken from the Berlin 'Jahrbuch' or made to depend upon that system.

In the first catalogue Mr. Porter carried the proper motion to only three places of decimals in Right Ascension, and two in Declination; in the latter catalogue he has added a figure in both determinations, though this does not, of course, imply any increased accuracy in the results. It is far more convenient for purposes of computation to have the number of figures in the proper motions the same as that for the precession. Mr. Porter's proper motions are the best collection and determination of proper motions of stars exceeding 0″·1, and are thoroughly excellent.

NOTES.

COMET NOTES.—(1) *Comet Holmes* (1892 *f* and 1899 *d*).—The very interesting periodic comet discovered by Mr. Holmes in 1892 has been detected on its return by Mr. Perrine at the Lick Observatory on June 10, the position being in very close accord with that predicted by Herr Zwiers in A. N. 3553. The following are his elements, the time of perihelion passage and mean anomaly being corrected by the aid of the recent observation. The correction to the former amounts to $+0^{\mathrm{d}}{\cdot}41$.

Epoch and osculation 1899 Sept. 9·0 Greenwich M.T.
M ...... 69142″·0
π ...... 345° 47′ 52″·9
☊ ...... 331 43 32·0 } 1899·0.
i ...... 20 48 9·9
ϕ ...... 24 17 21·9
μ ...... 516″·27
log a .... 0·55809
T ...... 1899 April 28·07 Greenwich M.T.

Ephemeris for Greenwich noon, corrected approximately by the recent observation.

	R.A.			N. Dec.			R.A.			N. Dec.	
	h	m	s	°	′		h	m	s	°	′
June 25....	1	38	37	21	42	July 19....	2	15	20	28	38
29....	1	45	0	22	53	23....	2	20	59	29	45
July 3....	1	51	18	24	3	27....	2	26	28	30	52
7....	1	57	30	25	13	31....	2	31	45	31	58
11....	2	3	35	26	22	Aug. 4....	2	36	50	33	4
15....	2	9	32	27	30	8....	2	41	43	34	9

The comet was faint on June 10, but its brightness is steadily, though slowly, increasing as it approaches the Earth, and by the middle of August it will be half as bright again as it was on that day.

(2) *Tempel's Second Periodic Comet* (1873 II. and 1899 *c*).—Ast. Nach. 3574 contains new elements and an ephemeris of this comet by M. Schulhof, corrected by the aid of the observations made last month. The apparition this year is a very favourable one, but nevertheless the comet will be at all times faint. It was of the 15th magnitude on May 6, and will be seven times as bright (the maximum) on July 27. In the middle of November the brightness will have sunk to half that on May 6.

Epoch and osculation 1899 Sept. 6·0 Paris M.T.

M	7°	21′	50″·5	
ω	185	36	20 ·0	1899·0.
☊	120	57	56 ·3	
i	12	38	52 ·6	
ϕ	32	49	38 ·7	
μ	671″·917			

Perihelion passage 1899 July 28·55 Paris M.T.

Ephemeris for Paris midnight.

	R.A. h	m	s	S. Dec. °	′		R.A. h	m	s	S. Dec. °	′
June 25....	20	8	17	7	23	Sept. 5....	21	38	29	36	0
July 3....	20	18	42	10	1	13....	21	49	45	36	14
11....	20	28	41	13	27	21....	22	1	24	35	46
19....	20	38	22	17	32	29....	22	13	16	34	47
27....	20	47	55	21	56	Oct. 7....	22	25	20	33	22
Aug. 4....	20	57	26	26	14	15....	22	37	34	31	38
12....	21	7	7	30	0	23....	22	49	54	29	42
20....	21	17	10	32	56	31....	23	2	17	27	36
28....	21	27	38	34	56	Nov. 8....	23	14	43	25	25

M. Schulhof points out that if a good series of observations of the comet is secured at this apparition it will enable us to improve our knowledge of the mass of Jupiter.

(3) *Swift's Comet* (1899 *a*).—This comet has been a very conspicuous object both during May and June, and has been distinctly visible to the naked eye.

The head of the comet was noticed to be distinctly double by Prof. Barnard at Yerkes on May 20 to 23. The distance between the components increased during this period from 29″ to 38″. A photograph taken by him on May 18 showed a slender tail some 8° in length.

The double nucleus was observed by Mr. Perrine a week earlier, the distance between the components being 12″ on May 11, and 18″ on May 14. The increased distance must be partly due to a real separation of the two portions (Ast. Journ. 464).

A notable increase in the comet's brightness took place on June 5, which was independently detected by a large number of observers (Centralstelle Circular, No. 21).

Prof. Hartwig says:—"June 5. There is no doubt the comet has increased in brightness; magnitude of nucleus 9½; whole comet about 5th magnitude. Greatest diameter of coma 12′."

Prof. Schorr says:—"June 5. Nucleus excentrically placed; 6½ magnitude; whole comet 5th magnitude. Diameter of coma 9′."

Ephemeris for Berlin midnight, by A. Stichtenoth (Ast. Nach. 3574).

	R.A.			N. Dec.			R.A.			N. Dec.	
	h	m	s	°	′		h	m	s	°	′
June 24....	14	33	43	25	44	July 18....	14	11	52	11	49
28....	14	26	3	22	26	22....	14	11	34	10	24
July 2....	14	20	38	19	39	26....	14	11	46	9	6
6....	14	16	54	17	16	30....	14	12	20	7	56
10....	14	14	20	15	13	Aug. 3....	14	13	15	6	52
14....	14	12	43	13	25	7....	14	14	26	5	54

The brightness during this period diminishes from ¼ to $\frac{1}{50}$ of that at discovery.

(4) *Coddington's Comet* 1898 VII.—Ast. Journ. 464 contains an ephemeris of this comet by C. J. Merfield. It must now be very faint, but possibly still within reach of large instruments.

Ephemeris for Greenwich noon.

	R.A.			Dec.			R.A.			N. Dec.	
	h	m	s	°	′		h	m	s	°	′
July 2 ..	4	50	53	0	49 S.	Aug. 19....	5	18	8	1	10
14 ..	4	59	40	0	4 S.	31....	5	21	1	1	19
26 ..	5	7	15	0	30 N.	Sept. 12....	5	21	53	1	25
Aug. 7	5	13	29	0	54 N.	24....	5	20	36	1	28

(5) *Barnard's Periodic Comet* 1892 V.—This comet was photographically discovered in 1892, and probably is a detached fragment of Wolf's Comet. The period is not known with accuracy, but lies between six and seven years. M. Coniel gives approximate search ephemerides in 'Bulletin Astronomique' for June; we give here the limiting points between which the search should be carried on (the places are for Paris midnight):—

Date.		R.A. h m	Dec. °		R.A. h m	Dec. °
July 29	From	22 57	45·1 N.	To	1 21	39·6 N.
Aug. 10		23 11	47·4		1 59	39·6
22		23 26	48·3		2 36	38·0
Sept. 3		23 40	47·3		4 52	19·8
15		23 54	43·5		5 21	14·0
27		0 8	36·0		6 14	3·8 N.
Oct. 9		0 24	24·3		6 31	2·3 S.
21		0 40	10·2 N.		6 12	6·8
Nov. 2		0 56	2·4 S.		6 16	13·6
14		1 14	11·2 S.		6 13	19·3 S.

(6) *Definitive Determination of the Orbit of Comet* 1854 IV.—Ast. Nach. 3571–72 contains a determination of the orbit of the above comet by Dr. C. Buschbaum and Dr. L. Steiner. The comet was independently discovered by six observers, the earliest being Klinkerfues in Göttingen, 1854 Sept. 11. It was under observation till Nov. 14. The following are the final elliptical elements after allowing for planetary perturbations:—

T	1854 Oct. 27·5510 Berlin M.T.
π	94° 22′ 51″·0
☊	324 27 11·9
i	40 53 43·6
$\log q$	9·902309
e	0·99245

Period 1088$^{\text{y}}$·8, with a probable error of 70 years.

A. C. D. C.

MINOR PLANET NOTES.—The following planets have received permanent numbers (Ast. Nach. 3570):—

Planet.	Discoverer.	Date of Discovery.	Number.
DP	Charlois	1898 July 16	437
DU	„	Nov. 8	438
EB	Coddington	Oct. 13	439
EC	„	Oct. 13	440
ED	Charlois	Dec. 8	441
EE	Wolf—Schwassmann ..	1899 Feb. 15	442
EF	„ „	Feb. 17	443
EL	Coggia	March 31	444

(439) EB has been named Ohio.

The planets provisionally designated DV, DW, DX, DY, DZ, EA, EM do not receive permanent numbers, not having been sufficiently observed.

The object EH has been struck out, being probably in error. The objects designated EG, EI, EK proved to be identical with (224) Oceana, (60) Echo, (222) Lucia. It was announced last

May that EK had been named Slatin; but this is now cancelled, both on account of its identity with Lucia and because the name is masculine. It has been decided both at Berlin and Kiel to recognize only feminine names for the minor planets, Eros being, of course, excepted (Ast. Nach. 3570).

Another planet, EN, was discovered by Witt on June 7, but this proves not to be new, but identical with (85) Io.

'Bulletin Astronomique' for June contains a table by Jean Mascart of the orbits of all the minor planets referred to the orbit of Jupiter instead of the ecliptic. This should be very useful to those engaged in the study of their perturbations. A. C. D. C.

THE LEEDS ASTRONOMICAL SOCIETY.—There is a small combination, deserving to be better known than it is, concerning which we cannot do better than quote from the 'Leeds Mercury' of 1898, May 14:—

> No organization is more economically managed, and none is doing a more acceptable work for its members, than the Leeds Astronomical Society. . . . . the Society has for the past five years, on an average income of less than £20 per annum, published annually Transactions of permanent value, and done much to redeem Leeds from the discredit of failure to support the former efforts of Sir John Herschel, Sir G. B. Airy, and a few local enthusiasts of the day, to establish in permanence an Astronomical Society in this populous and prosperous borough.

We are led to make this note just now because Mr. Washington Teasdale, who has been the mainstay of the Society for many years, and accomplished the above-mentioned feat of successful finance, has now retired from the Presidency. Mr. Teasdale is to be congratulated on leaving the Society in a prosperous and promising condition. The new President is Mr. C. T. Whitmell, under whose care the Society will equally flourish. The Journal of the Society for the year 1898, with which we have been recently favoured, is a well-printed book of more than a hundred pages, containing really excellent treatises on various subjects, and observations by members. Specially we were interested to see the portrait, taken by Mr. Teasdale and published in the Journal, of Mr. T. J. Moore, who by day works with his hands at agricultural labour, and at night measures plates of the Astrographic Chart taken at Oxford. He is photographed here in company with an Oxford micrometer, and six pages are usefully filled by Mr. Moore with a description of the system and progress of the Astrographic Chart and Catalogue.

A "FLAT EARTH PHILOSOPHER."—There is an entertaining gentleman, Breach by name, a town councillor of Portsmouth, who says, as some other persons have done, that the Earth is flat. We have seen leaflets by him before, but he has now obtained more notoriety than is possible from these self-distributed advertisements; for a paragraph in the 'Daily Mail' informs its readers that Mr. Breach has discovered an Act of Parliament, called the "Impostors' Act," which forbids anyone to teach that the Earth is round, under pain of punishment for schism, and that if the Portsmouth School Board do not desist from having this doctrine taught in the schools he will set the law in operation against them; and warns the London School Board to be on its guard.

An unexpected Sun-spot.—The sun-spot minimum still delays its arrival, and the Sun continues to make spasmodic efforts, like a weary listener to an endless tale, to convince observers that it has not yet passed into repose. Its latest exhibition of energy is still in progress. A regular spot of considerable dimensions was seen near the east limb on June 23, and passed the central meridian a little before noon on June 29. It will therefore have passed out of view at the west limb by July 6. The heliographic longitude of the chief spot was 330°, its latitude 5° N. Its area on June 29 was about 500 millions of square miles; that of the entire group was somewhat difficult to estimate, as the principal spot was followed by a number of faint markings which it was not always easy to distinguish from the ordinary photosphere. On June 25 and 26 these fainter markings formed a very curious semicircle round the *f.* portion of the chief spot, and concentric with it. Reckoning in these fainter markings the entire group on June 29 was about 87,000 miles in its greatest length, 27,000 miles in its greatest breadth, and with a total area of nearly 700 millions of square miles.

E. W. M.

A protracted Drought.—An unusually long period of dryness has recently been experienced at Greenwich. No rain fell from May 26 till June 17 (inclusive). During the period from June 18 till noon on June 30, the total rainfall amounted to 0·516 inch, which fell on 5 days. This is a small amount for June, as the average of 50 years gives 1·95 inch for this month. Since 1840 there have been only six occasions on which the rainfall for June was less than this, the least being in 1895, when the total rainfall was 0·21 inch.

The past month has seen two historic scientific celebrations—the Jubilee of Sir George Stokes at Cambridge, on June 1 and June 2, and the Centenary of the Royal Institution in the following week. There was nothing comprised in the proceedings at either function which was distinctly astronomical, except that some eminent foreign astronomers were present, as mentioned on page 276 of this number. But at Cambridge the Philosophical Society had a meeting, and received papers for a special commemorative volume of the Transactions, which comprise a memoir by Prof. Michelson on the Echelon Spectroscope, and a discussion by Prof. Schuster of the Magnetic Declination derived from Twenty-five Years' Observations at the Royal Observatory, Greenwich.

Many of the foreign guests who were present at these celebrations were entertained at Oxford on June 8. The honorary degree of D.C.L. was conferred on Prof. Newcomb and others.

It is announced that Prof. F. L. O. Wadsworth has been appointed by the Managers of the Western Pennslyvania University to succeed Prof. Keeler as Director of the Allegheny Observatory. It will be remembered that Mr. Brashear was in temporary charge of the institution.

The Adams Prize for an essay on the "Theory of the Aberration of Light" has been divided between Mr. J. Larmor, F.R.S., and Mr. G. T. Walker, Senior Wranglers in 1880 and 1889 respectively. We look forward with interest to the publication of these essays, which so much concern our science.

The partial solar eclipse on the morning of June 8 last was not observed at Greenwich, owing to clouds. A few reports of observations are to hand, but the subject is of small interest.

Errata.—May number, p. 212 line 43, for Hervey *read* Harvey; June number, p. 230 line 14, *read* difference of radii of the pivots multiplied by $\sqrt{2}$, if the cheeks of the V's are at right angles.

From an Oxford Note-Book.

At the beginning of this month (June) there was a most successful celebration of a Jubilee at Cambridge. Sir G. G. Stokes has been fifty years Lucasian Professor out of the eighty which he has lived; and a most distinguished company assembled at Cambridge to do him honour and themselves pleasure. In glorious weather it proved the easiest matter in the world to carry out both programmes. The veteran physicist himself probably enjoyed it all more than any of us. He heard lectures and speeches; received hundreds of addresses in the Senate House; attended a big luncheon, a bigger conversazione, at which he made a speech and had two busts of himself unveiled, and a huge dinner-party in the Hall of Trinity College, at which he again made a speech somewhere about midnight on Friday. And at noon on Saturday, which was the day for visitation of the Royal Observatory at Greenwich, who, do you think, was the first visitor to mount to the top of the new buildings at Greenwich with the elastic step of youth? Why, Sir George Gabriel Stokes, the octogenarian, who had slept for a few hours after all the festivities, got up in time to catch an early train, and was looking as fresh as paint. He had rather wickedly told us in the Senate House the day before that he might not have been there if he had not been careful not to *work too hard*. I wonder what amount of work Stokes would consider *hard* work?

An able article on "The Stokes Jubilee," by Prof. J. J. Thomson, in the *Cambridge Review* for June 1, may not have been seen by many of our readers; and I thus venture to give the following extract:—

> We can imagine no greater intellectual treat than to be one of the audience at his (Stokes's) professorial lectures, to admire the clearness of expression, the way in which difficulties are faced and removed, to see how everything becomes clearer after passing through his mind, and to enjoy his enthusiasm for his subject, an enthusiasm which is undeniable after fifty years' lecturing, and which led him last year, when a veteran of 79, to forget the time and continue lecturing uninterruptedly for three hours. In his lectures on light we marvel at the experimental skill with which the most difficult experiments on optics are successfully performed with the simplest apparatus; indeed, it has been said that if you give Stokes the Sun and three-quarters of an hour there is not an experiment in optics which he cannot perform.

In these notes for last January mention was made of the big model of the Moon which has been set up at Chicago after having been "held during ten years for storage charges." There is an article about it in *Nature* for June 8, accompanied by an illustration. I have not a copy of the Journal by me, but a selenographical friend tells me that, judging by the illustration, some of the pieces of the model must have been set up wrong—Fracastorius, for instance, being out of place.

Rather a new view of the relation of the Moon to the Tides was presented to me the other day. The question was asked—not at all aggressively, even perhaps deferentially—whether it was still considered probable that the Moon caused the Tides. I replied that, to the best of my belief, that was still the orthodox view; and asked in turn whether the enquirer had any serious reason for questioning the relationship. "Well, I don't know," he replied; "sometimes when there is no Moon there seems to be a tide all the same."

With regard to the note in the May number on the misuse of the name Cambridge I have received from the Editor of the *Astronomische Nachrichten* a letter explaining that he considered it so well known that there were two Cambridges, one of which was responsible for A.G. zone +25° to +30°, and the other for +50° to +55°, that he did not think any distinguishing mark was necessary. But if this amount of knowledge is to be assumed, surely the reference is altogether unnecessary? It would be sufficient to put simply A.G. zones. For myself I take a very different view of the situation: references of this kind *cannot* be two explicit; there is always somebody (if not in this generation, in a time to come) who does not, or will not, know the facts, and thus be misled by the use of the same name in two senses. For their guidance distinctions should be made as plain as possible—as, for instance, in the case of famous astronomers of the same name, the Herschels, the Struves, the Bonds. It is very little extra trouble or expense at the time and may save much confusion later.

Mr. Perrine has written the following letter concerning the actual mistake in the *Astronomical Journal* (in the *Ast. Nach.* there was no mistake, only a possibility of confusion), for which the responsibility has meantime been assumed by the Editor:—

> To the Editors of the *Observatory*.
>
> In the May number of the *Observatory*, p. 213. attention is called to an error in the authority for some of the comparison stars used in my observations of Comet 1898 I. and printed in No. 459 of the *Astronomical Journal*. The authority for stars 3, 4, and 5 is printed "Rogers, Camb. A.G. Catal." These stars are in the A.G. zone +25° to +30°, and are from the Cambridge, England, Catalogue of the A.G. Referring to my letter-press copy of the MS. of these observations I find that I gave the authority as follows:—" A.G. Cambridge, Eng." The erroneous change was made in the office of the *Astronomical Journal*, without my knowledge, as I did not see a proof before the number appeared.
>
> To prevent confusion, I gave the authority for the stars from the Harvard zone as " Harvard A.G." This designation was changed in the office of the *Astronomical Journal* to " Rogers, Camb. A.G. Catal."
>
> I very much regret these changes, for which I am in no way responsible.
>
> Lick Observatory, University of California, C. D. Perrine.
> 1899, May 17.

Now I have been an editor myself, and have the tenderest sympathy for editors into whose journals mistakes of this kind spontaneously intrude themselves. I can say from experience that it is scarcely ever the editor's fault, but (also from experience) that he has always to bear the consequences, unless he can adroitly put a brother-editor in front of him. So I don't want to make the consequences too serious, because Mr. Chandler only has two or three brother-editors to try to put the blame on, and they are all as excellent people as he is himself; but I put it to him or them as a fair question: If *he*, who lives in one Cambridge and knows of another, can make a muddle like this (is muddle rather too strong a word? perhaps they will forgive me), what can he expect from strangers who may not realize the duplicity of " Cambridge "? and is it not time something was done to remedy the evil? and might he not be the person to try and start the remedy?

The foregoing petition is made, and was made two months ago, in all earnestness. It may save misunderstanding to say this plainly, for these notes generally deal with topics in a lighter vein. But this warning may not after all be necessary, for I have recently had evidence that the discussion of serious matters is not considered to lie wholly without the scope of the Note-Book. During the last month two serious appeals have been made to me for the ventilation of grievances—one being, oddly enough, from a fellow countryman of Mr. Chandler, and another from the editor of an astronomical magazine such as his; and since I have asked attention to my little grievance, I cannot well do less than help others in similar straits. Moreover, I have every sympathy with the complainers in both cases; and although they are both doubtful as to the fairness of remaining anonymous, I think they need not have any scruples. One of them has since written giving me leave to disclose his name on demand by any aggrieved person; and as he attacks in a thoroughly straightforward manner a serious and growing evil (that of multiple publication of results), I have myself no doubt as to the propriety of publishing his letter. I would call special attention to the last sentence of it. The "instance used as a text" is only one among several more or less similar. The Secretaries of the Royal Astronomical Society, for instance, find it a continual source of anxiety to prevent the printing of papers which authors send out in duplicate or triplicate. There is no possible harm in this if they will pay the expense; but the funds available for scientific printing are strictly limited and are wasted by such a procedure. Or, again, if the same setting-up of type is made to serve for two journals (as in the duplicate publication by the Royal Society and the Royal Astronomical Society of Eclipse Reports), a larger public is reached without wasteful expenditure. But unless this waste is obviated in some such way, the growing practice of duplicate publication is certainly to be strongly condemned. Diogenes is no whit too strong in the remarks which follow:—

Mr. Oxford Note-Book,
Dear Sir,—

Sheets of galley-proof have recently been distributed, apparently broadcast, with the following "paster" attached thereto:—

> "Paper read before the Royal Society. To the Editor of the —— ——, for publication (with acknowledgment as a Communication made to the Royal Society); with the Author's compliments."

The paper on which this notice is printed does not blush at the breach of scientific propriety that it commits, and is apparently unconscious of insult to the Royal Society. Two meanings can be attached. One is that the author deems this Society to be so insignificant a body, and its 'Proceedings' so obscure a means of information, as to require a repetition of printing in alien sources. This implies lack of a proper sense of national dignity. The other is that he is so modest as to fear that his results will pass unnoticed and be lost to the world unless he secures their multifold presentation in as many journals as possible. This implies a lack of self-respect. That he may be able to find some editor who shares this feeling and who will assist him to avoid obscurity by reprinting the article, in no way palliates a violation of the recognized rules of good taste by the author.

It may be urged that this kind of offence against scientific good-breeding is not a new one, and that the author in question can find precedent and company. This is deplorably true. But it should be observed that it is neither good precedent nor creditable company. And something more needs to be said. The evil of duplicate printing of scientific communications has alarmingly increased in recent years. Bad precedents unfortunately establish themselves with fatal facility, partly through thoughtlessness, partly through familiarity with successive instances of departure from the canons of thorough scientific breeding. The process of degradation is gradual. The original offenders of course lost caste, but were rare enough to be looked at merely as sporadic cases of a want of sense of the fitness of things. Their followers in the succeeding generations are blameless to this extent, that they have followed the bad examples irreflectively, without discriminating between the kind of men among their elders who did this sort of thing and the kind of men who did not.

It is to be hoped that the instance here used as text will bring thoughtless sinners back to the true bearings, re-establish the sense of what is required by scientific dignity, and check the wastefulness of duplicate publication which is a growing evil of our time. DIOGENES.

MY other correspondent writes in more boisterous vein. He does not give me permission to disclose his name—I am not quite clear whether he quite wishes me to print his paper; but if he won't take credit for it himself, I will; it is too good to be lost, and American astronomers will be grateful to me for the self-sacrifice, for they must be heartily sick of the astonishing productions which the following note so cleverly satirizes. Let me assure them we do not, on this side of the water, hold them responsible in any way—we know they must be more disgusted than anyone, and "Dooley's" remarks may at least afford some relief to overtaxed patience with evil-doers.

On the Fundamental Law of Increase of Gaseous Reputation.

By DOOLEY.

As a matter that is destined to become of historic importance, it is desired to announce the following law, discovered April 1, 1899, by which the greatest phenomenon of the century is reduced to a formula, a rectangular hyperbola. This law overlays that of gravity itself, which, indeed, it upsets completely, no matter how seriously we try to consider it.

Let T be the scientific reputation that may be established by any means whatever, on the condition that it may be of a perfectly gaseous nature. Let J be the value which the investigator places upon popular newspaper or magazine notoriety, and J_1 that which he attaches to sound reputation as gauged by the good opinion of men whose good opinion is worth having. Further, let C be the amount of his egotism, not to be regarded as constant but as rapidly increasing at an enormous ratio, approaching asymptotically to the limit of mental alienation. Then we have

$$T = \frac{J}{J_1} C.$$

It is manifest that under the conditions named this function may assume an infinite value, as illimitable as the boundless sea, since it is composed of two factors, one a ratio which may become as large as we please by increasing the numerator and diminishing the denominator, each without limit, and the other factor, C, is a quantity that by hypothesis expands indefinitely as a necessary consequence of the operation of the law.

It does not become the discoverer to speak of the relative credit which should attach to the development of this far-reaching law and the finding of the comparatively small pebble on the beach of knowledge by a gentleman of the name of Newton some centuries ago; but it is evidently of the highest interest and the greatest advance in a difficult subject made in the last two hundred years.

If it be objected that the nature of this tremendous law has been known for the past thirty years, the discoverer must strenuously claim that he is the first to rescue it from oblivion, to reduce it to a formula, and to appreciate its vast significance and universality of application.

AT the end of last month's Notes an announcement was made by the Editors in a friendly way which I should not have ventured to think deserving of such attention; but it has brought us several of the kindest messages, for which we would ask permission to return sincere thanks.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY

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THE BRITISH ASTRONOMICAL ASSOCIATION.

The concluding Meeting of the ninth Session of the British Astronomical Association was held at Sion College on Wednesday, June 28th, *Mr. W. H. Maw*, President, in the Chair.

The election of nine new Members by the Council was confirmed by the Meeting.

The President asked Mr. Thomas Claxton, the Director of the Mauritius Observatory, who was present, to give some account of the work which was being done there.

Mr. Claxton said that the principal work was meteorology, magnetism, and the taking of sun-pictures. The meteorological work consisted of the ordinary observations taken four times a day, with photographic registration of the principal elements, and the collection of ship's logs for the determination of cyclone-tracks, and the preparation of synoptic charts, which, of course, were used in determining the tracks of the cyclones. The magnetic section comprised absolute observations of the three elements once a week and photographic registration of declination, horizontal force, and dip. The latest addition to the observatory had been a seismograph on the Milne principle, and this had brought to light some rather interesting features in the science of seismology. Mr. Claxton gave briefly some facts which appeared in his article in the July number of the 'Observatory,' and finally reminded the Meeting that there would be an eclipse of the Sun in 1901 and that Mauritius would be in the line of totality.

The President thanked Mr. Claxton for his interesting address and thought the distance ought not to prevent some members paying Mr. Claxton a return visit in 1901.

Mr. Downing asked what chances there were of clear weather in Mauritius in the month of May.

Mr. Claxton said he had not the figures with him bearing on the point, but the average amount of cloud and rainfall was very small in May.

Messrs. Gordon Miller and Henry Ellis were re-elected Auditors, and Messrs. T. Crawford, A. J. S. Adams, and E. Holmes were appointed Scrutineers of the Ballot for the new Council.

Mr. C. T. Whitmell, President of the Leeds Astronomical Society, read a short paper, illustrated by a simple apparatus, upon "Jupiter apparently Moonless." On August 21st, 1867, the planet for an hour and three-quarters appeared without any satellite separated from his disc. Satellites I., III., and IV. were in transit and II. was occulted. Before $8^h\ 14^m$ the order was I., IV., III., Jupiter, II. After $13^h\ 54^m$ the order was II., Jupiter, IV., III., I. From $10^h\ 4^m$ to $11^h\ 45^m$ the planet was apparently moonless. Satellites I., III., IV., and their shadows were all upon the disc from $10^h\ 4^m$ to $11^h\ 23^m$. A very remarkable series of solar eclipses would be in progress to observers on Jupiter on August 21, 1867.

Mr. Seabroke said he could well remember watching the satellites of Jupiter at the date mentioned by Mr. Whitmell, and although at the moment he did not remember the exact details they were of great interest.

The President said they were much indebted to Mr. Whitmell for his communications, giving them an insight as to what was going on in the solar system. He hoped Mr. Whitmell would continue his researches.

The President said that the Members had already received a list of the names of Members which had been nominated by the Council to be included in the ballot-list for the Council of next Session. Any member was now at liberty to propose any additions to the list. No nominations being made, the list as prepared by the Council became the ballot-list.

The President then announced that Mdlle. Klumpke, of the Paris Observatory, was present. She was so well known to all the Members that he need say nothing as to the attainments by which she had distinguished herself. She had kindly consented to say a few words as to what was being done in Paris.

Mdlle. Klumpke said it gave her much pleasure to be with them that afternoon and to convey to them the greetings of the Observatory of Paris and of the Astronomical Society of France. Mdlle. Klumpke then presented to the Association a copy of her lecture "La Femme dans l'Astronomie," delivered before the Astronomical Society of France in February last. Mdlle. Klumpke said they would be interested to know that the micrometric work in connection with the Photographic Chart of the Heavens was rapidly advancing at the Bureau of Measurements of the Paris Observatory. The positions of more than 217,000 stars of the zones allotted to Paris had been determined by her staff. The zones of declination $+24°$, $+23°$ were completed, and the zones $+22°$, $+21°$ were under examination. The stellar magnitudes were also being determined within one-tenth of a unit. This was done by means of a photographic scale giving standard images of the stars from the sixth to the twelfth magnitude. Mdlle. Klumpke then gave a short description of the giant telescope now being constructed by M. Gautier, of Paris, for the Exhibition of next year, and concluded by referring to the great loss that Astronomy and the Association had sustained by the death of Miss Brown.

The President thanked Mdlle. Klumpke for her interesting address and asked whether there was any radical difference between the system of measurement adopted here and that employed in Paris.

Mdlle. Klumpke said that at Paris they still adhered to the use of the filar micrometer.

The President then announced that *Father Cortie* had accepted the post of Director of the Solar Section and said that he was sure they could not have a more capable Member for the post.

Mr. Maunder said he was pleased to be able to present to the Meeting the completed Eclipse Report. They were having 1000 copies printed. The price would be 5*s.* net, but, according to promise, it was half-price to Members up to that evening. It was certainly a cheap book and contained nearly 200 pp. crown octavo, beside some 60 illustrations. Mr. Maunder added that they had made some progress in their arrangements for the expedition to Spain and Algeria in May 1900. So far as could be estimated the rates for Members for passage would be: to Lisbon (or Oporto) and back £10; to Cadiz, giving access to the centre of Spain, and back £14; to Alicante £16. The cost for the entire trip for those going to Algiers and remaining on board during the stay at Algiers would be £22 10*s.*

Mr. Crommelin submitted an abstract of a paper by *Mr. Grigg* dealing with the determination of ephemerides of periodical Comets, and *Mr. Maunder* presented the interim report of the Mars Section, prepared by *M. Antoniadi*, the Director. The Meeting was then closed at 7 P.M.

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*The Work of Women in Astronomy.**

THE history of women's work in Astronomy presents three great phases. The first goes back to the remotest antiquity, and ends with the dawn of the Renaissance; the second begins with the revival of astronomical science and ends with the early part of the nineteenth century; the third begins at the birth of photographic and spectroscopic astronomy, and comprises the history of the women astronomers of our day. Each of these three phases has its distinctive features, determined by the conditions under which women laboured, by the instrumental means employed, the work accomplished, and by the results obtained from a scientific, a practical, and a moral point of view.

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* Read by Miss Dorothea Klumpke before the International Congress of Women in London, 1899 July 29.

During the first phase astronomy was in its infancy. Erroneous ideas as to the dimensions and distances of the heavenly bodies then prevailed; man, in his pride and ignorance, thought the Earth to be the centre of the visible universe; even they who, by daily observations, were brought into closer contact with the celestial bodies, little understood the moral beauties of astronomy, which science was then chiefly used for self-advancement and political purposes: this was the era of superstition. The instruments of the first period, though of a rudimentary form, were beyond the people's reach; all observations were made with the naked eye.

To the majority, and to women in particular, astronomy must have been purely contemplative, for then, as to-day, the glory of the heavens spoke to the soul of woman, which has never changed. Contemplation soon gave place to observational and educational astronomy; woman's interest had been aroused and her energies awakened; indeed Plutarch mentions *Aganice*, an Egyptian princess, who predicted future events by the relative positions of the celestial bodies. In Thessaly, *Aglaonice* announced lunar and solar eclipses; *Asclepigenia*, in Athens, and later *Hypathia*, in Alexandria, imparted their knowledge to young and old, and in the silence of the night Hypathia meditated on the motions of the heavens. But, as was the astronomy of the early ages, so also was woman's longing for truth misunderstood; it was met with derision and scorn, but bravely woman fought against tyranny, jealousy, and superstition; by martyrdom she sanctified the noble aspirations of her soul.

The second phase in woman's work in astronomy covers the fifteenth, sixteenth, seventeenth, and eighteenth centuries. The walls of the prison wherein narrow and prejudiced intellects would have kept the most noble of sciences fell at the word that announced the discoveries of Copernicus, Galileo, Kepler, and Newton. The Earth is no longer thought to be the centre of the universe. It is but one of the secondary bodies circling round our Sun, whence it derives its light, heat, and vitality. Compared with the innumerable hosts of bodies disseminated throughout space, the planet upon which we dwell is but a grain of dust in the universe! In this period the telescope enhances the astronomer's vision; celestial mechanics are founded on the law of gravitation. Henceforth planets and comets no longer escape the astronomer's mind; though he may not perceive them with his bodily eye, yet he knows their position in space. By observation, by the aid of analytical formulæ, by tables of logarithms, he computes for long intervals of time, and with an accuracy that would have seemed incredible to the astronomers of old, ephemerides of the various members of our solar system. In this important labour of computation and observation the women astronomers of the second phase rendered most useful service.

We find them at work in Germany, France, Italy, England; but few were the women whom their environment enabled to partake of the pleasures of astronomy and to produce intellectual labour. Therefore this second phase of woman's work in astronomy is the period of astronomical aristocracy.

Marie Cunitz, in Germany (1610–1664), heads the list of these gifted women. In 1650 she published, under the title of 'Urania Propitia,' a compendium to the Rudolphine Tables, which testifies to the genius of a woman upon whom her contemporaries bestowed the appellation of "Second Hypathia."

Helvetius's devoted wife and assistant for more than twenty-seven years observed with her husband at their private observatory in Dantzic, and prepared with him the elements of the 'Prodromus Astronomicæ' and of their Catalogue of 1883 stars.

Margarethe Kirch, aided by her scientific family, computed astronomical ephemerides and almanacs for Berlin, Breslau, Dresden, and Nuremburg. Margarethe Kirch's tribute to science equals her tribute to humanity; in her Science honours the gifted computer, and posterity the mother of one of the first directors of the Berlin Observatory. Many years later, in Germany also, *Frau Rümker*, who discovered a comet in 1817, shared the scientific labours of her husband, Director of the Hamburg Observatory.

In France, during the period in question, many women won public recognition: *Jeanne Dumée* for her Commentary of Copernicus's memorable work 'De Revolutionibus Orbium cœlestium'; the *Marchioness du Châtelet* (1706–1749) for translating Newton's 'Principia'; *Madame Lepaute* for determining with Clairault the orbit of Halley's Comet, and for computing for more than ten years the astronomical tables of 'La Connaissance des Temps'; *Madame Dupierry* for computation of lunar eclipses and for lectures on astronomy delivered in Paris; *Jeanne Amélie Lalande* (1768–1832) for reducing the positions of the 10,000 stars which form her husband's catalogue, and for computing astronomical tables for the determination of time at sea. To this period also belongs *Madame Villarceau*, the astronomer's wife, who computed orbits of binary stars after having verified the analytical expression of the formulæ employed.

In Italy, women astronomers are represented by *Teresa* and *Madeleine Manfredi*, the sisters of the director of the Observatory of Bologna, to whom Science is indebted for solar, lunar, and planetary ephemerides. Toward the close of this second phase *Signora Scarpellini* (1807–1873), the niece of the founder of the Capitoline Observatory, represents woman both in astronomy and meteorology. Signora Scarpellini not only discovered a comet and observed the Leonids' shower of 1866, but she also published a monthly journal and established a meteorological station in Italy.

In England, a most beautiful instance of sisterly love joined to love of science is given to the astronomical world by *Caroline Herschel*, the little, quiet, home-bred Hanoverian girl, "of whom nothing had been expected," says Mrs. John Herschel, "but to be up early and to do the work of the house, and to devote her leisure hours to knitting and sewing. With unvarying diligence and humility, Caroline Herschel followed the path that her brother had marked out for her, first, in blessed hourly companionship, when she was as necessary in his home as in his library or among his instruments, and latterly, when with saddened heart, but unflagging determination, she continued to work for him, but saw his domestic happiness pass into another's keeping."

Beside the work done in collaboration with her brother William, astronomy is indebted to Caroline Herschel for the discovery of eight comets and for many of the nebulæ contained in Sir William's Catalogue. In 1797 she presented to the Royal Society a catalogue of 560 stars, and after the death of her brother, Caroline, then 72 years of age, undertook and completed the laborious reduction of the places of 2500 nebulæ to the 1st of January, 1800, "presenting in one view," says T. South, "the entire results of Sir William's observations on these bodies, thus bringing to a close half a century spent in astronomical labour."

Caroline forms the connecting-link between the second and third periods of woman's work in astronomy. Like the women of the second period, she is related to an illustrious astronomer; through privation of fortune she belongs to so many of the toilers of the third period who have found in astronomy a modest means of livelihood. The appointment of a young woman as assistant astronomer, with a regular salary for her services, had not been heard of until then.

In this second period, the results obtained by the women astronomers are truly scientific. They must have been practical also, from a financial point of view, as the publication of almanacs was a source of revenue; lastly, the above-mentioned contributions had a moral and beneficent influence upon the soul, as testifies the 'Urania Propitia,' throughout which book ring the praises of the Most High.

In the third phase of woman's work in astronomy, a new element—*equality*—appears. Social evolution, like agitated waves, has touched the confines of many lands—its ebb bearing away vicious abuses, its flood rolling in crested with precious hopes. Instruction has become obligatory; intellectual food is given to hungry man and woman, and while man's heart draws closer to nature, nature draws closer to man, revealing to him the great unity of principle that exists in all sciences and links them intimately together. In this period, astronomy no longer limits her researches to the determination of the positions of the stars and planets on the blue firmament! Optics, physics, mathematics, chemistry, photography have transformed her in her very foundations, opening to her wider fields, and lavishing upon her the benefits of their discoveries. Instruments of all descriptions stand at the command of astronomy! the photographic eye and the human eye share the work of fathoming the heavens.

Numerous branches appear in astronomy—photographic, spectroscopic, theoretical, numerical, historical, literary, observational, and educational astronomy. So rich is the harvest's yield, and so short is human life, that astronomical science now becomes universal! she knows no boundaries, no rank, no sex, no age! In these various branches the women of the nineteenth century have their representatives. In photographic astronomy women in the national observatories of Paris, the Cape of Good Hope, Helsingfors, Toulouse, Potsdam, Greenwich, Oxford, have contributed to the International Photographic Chart of the Heavens. Day by day, women bending over the micrometers examine the photographic skies and measure the star-positions to form the catalogue which is to be a legacy of our century to future generations. At the Paris Observatory during the last seven years, the positions of 21,700 stars have been determined by the Bureau of Measurements, placed, in 1892, under my direction by the late Admiral Mouchez, the promoter of the photographic survey of the heavens.

Spectroscopic astronomy is represented by *Lady Huggins*, the collaborator of a world-renowned astronomer. At the Harvard College Observatory a staff of women, directed by *Mrs. Mary Fleming*, pass in review millions of stars photographed as bands of light, the dark lines characterizing the star's chemical composition found in the stellar spectra are identified with the corresponding lines of terrestrial substances. The stars are thus classified according to their probable evolution. Careful examination of the spectral lines has led the Harvard College staff of women workers to the discovery of many variable stars and binary stars, too close to be revealed by the telescope.

In theoretical astronomy woman was represented, a decade ago, by *Sonia Kowalewski*, late Professor at the University of Stockholm, author of an analytical paper on the rings of Saturn, laureate of the Bodin prize, and whose sad and premature death cut short a most brilliant career. In England this branch was represented by *Mary Somerville*, whose beautiful book on the mechanism of the heavens has been spoken of in the highest terms by Biot and Humboldt. In numerical astronomy woman, by her talent and industry, has forced the doors of the office of the American Almanac. In historical and literary astronomy woman has surpassed herself in the person of *Miss Agnes Clerke*, the author of a 'Popular History of Astronomy during the Nineteenth Century,' of the 'System of the Stars,' of the 'Herschels and Modern

Astronomy,' and of many valuable contributions, crowned in 1893 by the Acton Prize. In observational astronomy women work with equal zeal and perseverance in the national observatories, in women's colleges, and at their private telescopes.

At a few of the national observatories woman has observed planets, comets, and shooting-stars; she has been in charge of the time service and has made transit observations. In the American women's colleges at Vassar, at Smith, at Swarthmore, women professors, following the footprints of *Maria Mitchell*, their illustrious predecessor, guide their pupils in practical and theoretical astronomy. At her private observatory, woman has had a worthy representative in the late *Miss Elizabeth Brown*, Director of the Solar Section of the British Astronomical Association, to whom the founding of this Association was due in a large measure.

In the British and in the American and French Astronomical Associations, women to-day fill the functions of observers, of councillors, of secretaries, of vice-presidents, of benefactresses. Her work is done gratuitously; positions are open to all, the discipleship of Astronomy being the only recognized title of nobility.

Unable, for want of time, to go deeper into my subject, I beg to refer my audience to a list of the services rendered by the women astronomers of our day. I wish to add here that among the toilers of the various branches mentioned above are found the qualities requisite for producing lasting results—concentration and enthusiasm, powerful levers that move worlds!

Ours is a work of the night and the day! incessantly we sow, we toil, we reap; gratefully we gather in the full sheaves of the harvest due to the valiant men and women of the past who have opened to us a new liberal profession. Lovingly, we confide to the soil of intellectual fraternity our little grain of labour, trusting it may yield a hundredfold return to science, to humanity, and especially to womanhood of the centuries to come! Perseveringly we labour, taking little heed to the cares of the morrow, knowing that astronomy, like spiritual understanding, bears within her her own blessings. "She is more precious than rubies and fine gold; length of days is in her right hand, and in her left riches and honour."

DOROTHEA KLUMPKE.

A possible Cause of the Gegenschein.

In the 'Observatory' for November 1898 I suggested that the gegenschein might possibly be but a contrast effect produced by the shadow of the Earth projected upon the luminous matter comprising the zodiacal light, but sufficiently reliable data were not then available to enable me to pursue the subject further. The publication by Prof. Barnard in 'Popular Astronomy,' no. 64, of his observations of the gegenschein enables me to present my hypothesis in a more detailed manner, which I propose to do in this paper, in which, as before, I assume that the zodiacal light is produced by reflection of the Sun's light from particles of matter arranged in the form of a thin disk or ring lying in, or nearly in, the plane of the ecliptic, the Sun being at or near its centre, and the Earth immersed in it not very far inside its outer edge.

According to Prof. Barnard the gegenschein

(*a*) is in general circular in outline, and sometimes as much as 20° in diameter;

(*b*) four times a year, at intervals of three months, viz., in January, April, July, and October, it appears elongated;

(*c*) much of the time it is intimately connected with the zodiacal band;

(*d*) "it doubtless lies exactly in the ecliptic and always exactly opposite the Sun";

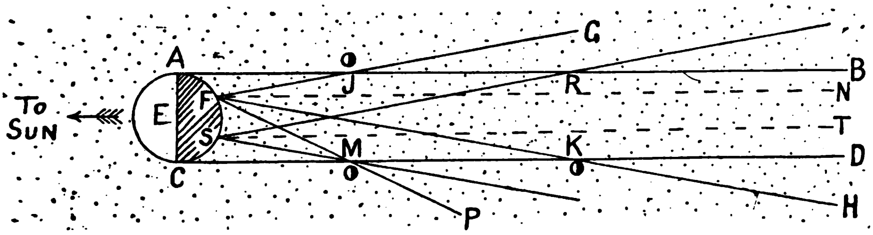
(*e*) it is "very feebly brighter in the middle";

(*f*) it "does not seem to be extremely far away";

(*g*) it has no appreciable parallax.

Any hypothesis, therefore, must account for these facts.

Let the accompanying figure represent a section at right angles to the ecliptic through the Earth and a portion of its shadow, and of the disc of matter composing the zodiacal light, E representing the Earth, and AB and CD the edges of a portion of its shadow. These lines are, of course, supposed to meet at a point far out on the right, and the disc of particles is supposed to be much thicker from its north to its south side than here illustrated. The particles of matter within the Earth's shadow are invisible, and an observer at F, looking in the direction FN, is looking up the inside of a long dark cone surrounded by the faint light reflected from the particles.



Suppose the light from a particle of matter at J (an enlarged "particle" is shown at J and at M and K)—which particle is very nearly full to the observer at F—backed up by the light from the particles lying along the line JG, to be only just visible from

F, then the light from all the particles lying outside, *i.e.* above, FG will be quite invisible, partly because those particles nearer to A than is J will present a smaller illuminated surface to him, and partly because the line of sight will soon pass out of the disc of matter to the north of the ecliptic. But the light in the direction FP will not be visible to the observer, because the particles at M present an illuminated area to him smaller than those at J, and also because the line of sight FP will soon run out of the disc of matter at P, on the south side of the ecliptic. It is only when he looks in the direction FH, where the angle DKH is equal to the angle GJR, that the fullness of the particles at K and the light from those in the direction KH combine to render the light equal in brightness to that seen in the direction FG. The contrast between the darkness within the shadow and the light surrounding it will also help to make the particles immediately about the shadow visible.

To the observer at F the gegenschein will therefore be included within the angle GFH, and the line FN bisecting this angle lies in the plane of the ecliptic, and therefore the centre of the gegenschein lies on the ecliptic. The same considerations apply in the case of an observer at any other situation, say one at S. In his case the light will be visible in the direction SM, but not in the direction SJ. Only when he looks in the direction SR will any light be seen. The gegenschein will therefore to him be included in the angle RSM, and as the line ST, bisecting this angle, lies in the plane of the ecliptic, the centre of the gegenschein will also be on the ecliptic as seen from S. Thus the gegenschein will show no parallax, even although some of the particles of which it consists are quite close to the Earth. To the observer at F the particles composing the boundary of the gegenschein will be those lying in an ellipse around the Earth's shadow, the longer axis of which will be JK, while to the observer at S the ellipse will have MR for its longer axis. As the Earth rotates these ellipses will change their form from hour to hour, and will only become circular if the observer have the ecliptic in his zenith at midnight; but the ellipses will always appear approximately circular, and their centres on the ecliptic, no matter what their actual shape or the position of the observer may be. Conditions (*a*), (*d*), (*f*), and (*g*), given above, are thus fulfilled.

Looking at the gegenschein nearer to its centre than R or K, the line of sight will pass through a much greater number of particles; but their light will not increase in proportion to their number, because of their diminished size through distance, and because of the occultation of some of the more distant smaller particles by the nearer and therefore larger particles, and also because the entire disc of particles probably becomes more tenuous near its outer edges, in addition to which these further particles lie behind a considerable mass of dark particles within the Earth's shadow. The gegenschein will therefore probably fulfil condition (*e*) by being but slightly brighter towards its centre.

Looking east or west along the ecliptic from the point opposite the Sun, the observer at F will not see the light from particles so close to him as those at J and K because of want of contrast with the Earth's shadow, and because every particle will then turn to him a smaller portion of its illuminated hemisphere, and his line of sight will only pass through a number of particles sufficient to render their combined light visible when it makes a much smaller angle with the ecliptic than FG or FH. Thus east and west of the anti-sun the visible light from the disc of particles will be included in a much smaller angle than the gegenschein, and will appear as a zodiacal band stretching along the ecliptic to the east and west of it, and so satisfying condition (*c*).

The second condition, (*b*), is perhaps the most difficult to comply with in a satisfactory manner, and it is not without some diffidence that I offer the following explanation. The fact of the elongation occurring at intervals of three months would seem to point to some variation in the arrangement or density of the particles in the disc which the Earth encounters at four more or less definite positions in its orbit. If the disc be supposed to consist of a series of concentric rings, alternately dense and tenuous, but differing little in density, and not concentric with the Earth's orbit, then, when the Earth is immersed in a dense ring, the gegenschein will tend to appear elliptic in outline, because the density of the matter will cause the gibbous particles close to the east and west sides (along the ecliptic) of the Earth's shadow to become visible by their combined light, while an equal augmentation of the light will not occur to the north and south of the ecliptic, for reference to the figure will show that as the line of sight FG, for instance, makes a greater angle with the ecliptic it passes through a rapidly *diminishing* number of particles, while as the line of sight carried along the ecliptic, instead of at right angles to it, makes a greater angle with the anti-sun, it passes through an *increasing* number of particles. But when the Earth would pass into a less dense ring, the particles around the shadow at J and K might still be visible to the observer at F, seeing that their visibility depends more upon their nearness and fullness, combined with the contrast produced by the Earth's shadow, than on their numbers; while those particles lying east or west of the shadow might, from their tenuity and gibbosity and want of contrast, be too faint to be visible. But the particles in the next ring beyond—a dense ring—would constitute a zodiacal band which, if visible at all, would be narrow by reason of its distance, and would come up to the circular gegenschein on the east and west without any apparent increase of breadth.

Saturn's rings afford a precedent for regarding it as not improbable that zonal variations in density would occur in a ring or disc of matter revolving around the Sun, and in the current (June) number of the 'Observatory' I have given my reasons for thinking that the Earth's orbit is not concentric with the disc.

I have now attempted to account for all the observed facts mentioned at the beginning, and it would thus appear that a disc or ring of matter such as I have here taken for granted is capable of producing at the same time the zodiacal light, the band, and the gegenschein. The size and form of the latter would probably vary according to the density of the matter or the thickness of the disc, and it might even be affected by the atmospheric conditions prevailing around the twilight zone.

The suggestion which Prof. Barnard mentions (in 'Popular Astronomy,' no. 64) as a possible explanation of the gegenschein appears to postulate a greater density than seems to be admissible in a ring or zone of matter thousands, if not millions, of miles in thickness, whether it be asteroidal or not.

Although the foregoing perhaps reads more like an attempted demonstration than as a presentation of an hypothesis, it is, of course, as the latter that it is offered, in the hope that, should it be untenable, it may at least lead to something better. In dealing with an object bordering upon the limit of visibility, nothing tending to increase or diminish its brightness in the very least degree may be overlooked, and, as seasonal changes as regards transparency probably recur in most, if not all, places, too much reliance should, perhaps, not be placed upon a series of observations made at any one situation, no matter how careful or accurate such observations may be. In this paper I have only used the results of Prof. Barnard's excellent series of observations, but when due allowance has been made for the effects of situation and personal equation, most, if not all, of the observations known to me appear to be fairly explicable on the above lines.

Madeira, 1899, June 12.

Wm. Anderson.

On the Tables in the Prayer-Book for finding Easter.

Easter-Day is always the first Sunday after the Full Moon which happens upon or next after the twenty-first day of March; and if the Full Moon happens upon a Sunday, Easter-Day is the Sunday after.

The Paschal Full Moon, according to the above rule, cannot happen earlier than March 21, and since the length of a lunation is about 29 days, it cannot fall later than April 18. Easter-Day, therefore, must fall between March 22 and April 25, both days inclusive. It will fall on March 22 if the Paschal Full Moon happen upon March 21, being a Saturday, and on April 25 if the Paschal Full Moon happen upon April 18, being a Sunday.

Easter-Day must therefore be one of 35 days; but the consideration that Easter-Day must be a Sunday at once reduces the number of possible days in any particular year from 35 to 5. In the year 1899, March 26, April 2, 9, 16, 23, were Sundays, and consequently Easter was bound to fall upon one of these days.

The operation of finding the Sunday letter, for which the rule is given in the Prayer-Book, is designed to ascertain rapidly which days in any particular year are Sundays. In the year 1820, April 2 was a Sunday; to find the day of the week on which April 2 fell in any other year of this century (exclusive of 1900), it is sufficient to note that April 2 falls one day of the week later every common year, but two days later in a leap-year. The rule for April 2 would therefore be:—"Subtract 1820 from the number of the year, and add to the number so formed its fourth part, omitting fractions: divide the sum by 7 and add the remainder in days to Sunday." Now as 1820 is a multiple of 28, it is unnecessary to subtract it from the number of the year, and the rule may be revised as follows:—"To the number of the year add its fourth part, omitting fractions; divide the sum by 7 and add the remainder in days to Sunday." April 2 can only be a Sunday therefore (during the century 1800–1899), if the remainder so formed is zero, or, in the language of the Prayer-Book, where the letter A is placed opposite the remainder zero, if the Sunday letter is A. This is why the letter A, besides being placed opposite the remainder zero, is also placed opposite April 2, and also opposite March 26, April 9, 16, 23. It means that when the Sunday letter is A, or, in other words, when the remainder is zero, the above-mentioned five days are Sundays.

As we pass from 1899 to 1900 the number of the year increases by one, and its fourth part, omitting fractions, also increases by one, and therefore the remainder after dividing by seven is either two greater or five less. But as leap-year's day is omitted according to the Gregorian calendar in the year 1900, Sunday only falls one day earlier, corresponding to an increase of one and not two in the remainder. Consequently it is necessary to subtract one from the remainder formed as before during every year from 1900 to 2099; then, owing to the year 2100 being a common year, we must subtract two for the next hundred years, and so on, a table in the Prayer-Book showing exactly what has to be done up to the year 8599, which is sufficient for all immediate purposes.

So far no knowledge of astronomy has been required. We are merely concerned with the day of the week corresponding to any date in the Gregorian calendar. No doubt the Gregorian calendar is based upon the length of the year as determined by astronomy; but the Prayer-Book tables are not designed to justify the Gregorian calendar, but merely to find the date of Easter according to the Gregorian calendar, be that calendar astronomically right or wrong.

The Golden Numbers are used to find the date of the Paschal Full Moon. When this date has been found, the following

Sunday, recognized by the letter against the date being the Sunday letter corresponding to the year, is Easter-Day. The golden number is the remainder, increased by unity, obtained by dividing the number of the year by 19. Consequently any year will have the same golden number as the nineteenth preceding or succeeding year; and if two years with the same golden number fall in the same century, the date of the Paschal Full Moon is the same for each. This implies that 19 years are very nearly equivalent to an exact number of months. This, of course, is not quite accurately the case, but the rule is clearly a useful one if it takes a century or two for the discordance to accumulate to an entire day; when it has so accumulated the error is easily put right by shifting the date of the Paschal Full Moon corresponding to a particular golden number. For instance, 14 is the golden number for 1704, 1894, and 1913. Consequently for 1704 and 1894 the date of the Paschal Full Moon is March 21, implying that the interval from 1704 March 21 to 1894 March 21 is sufficiently nearly equal to an exact number of months; but the tables in the Prayer-Book give March 22 as the date of the Paschal Full Moon for 1913, implying that the 209 calendar years from 1704 to 1913 fall short of an exact number of calendar months by about one day. A glance at one of the tables in the Prayer-Book shows that the difference is supposed to accumulate to one month in the 69 centuries between 1600 and 8500, the calendar years falling short. If Gregory had not struck leap-year's day from 52 of these years, the difference would have been about 22½ days the other way. We may infer, therefore, that the Prayer-Book considers that 6900 Gregorian years are too short by about 29½ days, or that 6900 Julian years are too long by 22½ days, or that 19 Julian years are too long by six-hundredths of a day, or that

$$19 \times 365{\cdot}25 - 0{\cdot}06 = 6939{\cdot}69 \text{ days}$$

is an exact number of months. This exact number of months being 235, it follows that the length of the month according to the Prayer-Book is 29·5306 days, correctly to one ten-thousandth part of a day. This is in accordance with the best astronomical determination.

Twelve months contain 354·37 days, and thirteen months contain 383·90 days. Consequently twelve months fall short of an ordinary year by a little less than eleven days, or of a leap-year by a little more than eleven days, and thirteen months are in excess of an ordinary year by nineteen days, and of a leap-year by eighteen days. Therefore during the course of a cycle of nineteen years entirely included in one century, the Paschal Full Moon falls eleven days earlier than in the year before on twelve occasions, nineteen days later on six, and eighteen days later on one.

When the Paschal Full Moon of a leap-year falls nineteen days later than the Paschal Full Moon of the preceding year, the interval is 385 days, and consequently over a clear day wrong;

but this objection is unavoidable, except by replacing the existing system by a different and more complex one. Care has evidently been taken in constructing the tables for different centuries that the interval is never wrong by more than one day and a tenth. In fact when there is a change at the last year of a century, the days attached to all or all but one of the golden numbers move forward one day, turning in effect the usual common year into a leap-year if the Paschal Full Moon happen on one of the golden numbers so moved forward; or in a few rare cases the numbers are moved backwards, turning in effect the leap-year common to both Julian and Gregorian calendars into a common year if the Paschal Full Moon happen on one of the numbers so moved backwards. P. H. Cowell.

The Observatories of India.

A question of some importance to the progress of Astronomy now being discussed in official circles is the future direction and the programme of work of the Observatories which are under the control of the Indian Government. It may be said, without imputing blame to anyone, that none of these establishments have a sufficient equipment, or have issued results at all adequate to the clear skies that they enjoy. As long ago as 1883 the Astronomer Royal was consulted by the Secretary of State for India as to the future maintenance of the Madras Observatory, and as an effect of Mr. Christie's advice a Committee of the Royal Society now exists to whom reports are annually made by the Directors of the Bombay and Madras Observatories. In 1897 the Secretary for India, knowing that the Astronomer Royal was going to that country to observe the Eclipse of 1898 January, asked him to visit certain observatories with a view to reporting or for drawing up a scheme of re-organization; and a similar request was made to Sir Norman Lockyer. The summary of the Astronomer Royal's report is given below, but it may be well first to give some little account of the observatories as they exist at present.

The Madras Observatory was established in 1792 under the direction of Mr. Goldingham, and besides some miscellaneous observations by that astronomer, a large amount of meridian observing has been done here under successive Directors; but unfortunately the reduction of the observations did not proceed *pari passu* with the observing, and there were large arrears which are only just now being overtaken, thanks to strenuous efforts. The Bombay Observatory at Colaba was established by the East India Company in 1826, and has been devoted almost entirely to magnetic and meteorological observations. The Astronomer Royal states that the site is most unfavourable for either science, but that the work from this observatory forms a valuable contribution to terrestrial magnetism. There is a transit instrument here, and Time is supplied to the shipping in Bombay harbour. At Dehra Dûn there is an observatory under the direction of the Surveyor-General's department, where for many years past daily photographs of the Sun have been taken under the auspices of the Solar Physics Committee, but it seems that the taking of large-scale photographs (12 inches to the Sun's diameter) has been abandoned. Besides these there is an observatory at Calcutta where magnetic records are made and also time determinations, and another at Simla, where the chief subject of observation appears to be the Sun's radiating power: and there is also an observatory attached to the Royal College of Science, Poona, where Prof. Naegamvala undertakes researches in Solar Physics in addition to his duties as Professor of Physics in the College, without remuneration. Last, but not least, there is the beginning of an observatory at Kodaikanal, in the Palni Hills. On the death of Mr. Pogson in 1891, who had been Director of the Madras Observatory, the Indian Government determined to enlarge the scope of that institution and to create as an adjunct, or possibly to substitute for it, a larger and better situated observatory. Mr. Michie Smith was appointed Director and the Kodaikanal Observatory is being built under his supervision. In his Report for the year 1893 to the Secretary of State, the Astronomer Royal stated his opinion as to the value of continuing to make observations for fundamental astronomy of position in India, as well as to study solar physics, for which the climate is peculiarly suitable. There has also lately arisen a feeling that the time has come when a magnetic survey of the country should be undertaken. It will be seen that this is provided for in Mr. Christie's summary, which runs as follows—

1. Provision to be made for astronomical observations (positions of Sun, Moon, Planets, and fundamental stars) and solar physics observations at Kodaikanal, under the Government Astronomer of India, with a Deputy Astronomer and a staff of native assistants.
2. The daily photographs of the Sun to be continued at Dehra Dûn under the Surveyor-General of India, at any rate for the next five years.
3. The proposed Magnetic Survey of India to be carried out by the Survey of India Department under a Superintendent, who should have a special knowledge of terrestrial magnetism.
4. The Madras Observatory for time and meteorological observations to be placed under a half-time officer as Superintendent, with a reduced staff (as in Mr. Eliot's scheme), the whole work being under the direction of the Government Astronomer of India.

5. The Bombay Observatory for magnetical and meteorological and time observations to be also under a half-time officer as Superintendent, with a somewhat reduced staff (as in Mr. Eliot's scheme), the whole work being under the direction of the Superintendent of the Magnetic Survey.
6. A small Board of Visitors, composed of the Surveyor-General, the Meteorological Reporter, and one or two other officials selected by the Indian Government, to make an annual inspection of Indian Observatories, and report to the Indian Government on their condition and administration.
7. An annual report by the Government Astronomer of India, and by the Superintendent of the Magnetic Survey (through the Surveyor-General), to be submitted to the Observatories Committee of the Royal Society.

Sir Norman Lockyer's proposals do not differ greatly from these. In his Report no reference is made to Astronomy of Position. He is of opinion that the daily photographic record should be continued at Dehra Dûn, and makes some vigorous remarks as to the discontinuance of the taking of the 12-inch Sun-pictures. Especially, he lays down a programme of work for the Kodaikanal Observatory, which would make it wholly a Solar-Physics Observatory. Sir Norman states that the plans of this Observatory require considerable alteration, and urges that the solar-physical work should be at once begun in temporary sheds; in fact it should have been begun some time ago. It is not difficult to read between the lines of his report that Sir Norman considers Prof. Naegamvala's claims to be Director of the Solar-Physics Observatory should not be neglected.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Dörfel and the Great Comet of 1680.

GENTLEMEN,—

It is well known that, a few years before the publication of the 'Principia,' Georg Samuel Dörfel (as the name is commonly spelt), of Plauen in Voigtland (an old designation of a part of Saxony), concluded from his own observations of the great comet of 1680–1 (sometimes called Newton's comet) that it moved in a parabolic orbit with the Sun in its focus. But, of course, this no more anticipates Newton's theoretical proof that planets and comets move in conic sections round the Sun than did Kepler's discovery that Mars moves in an elliptic orbit with the Sun in one of its foci. Dörfel's tractate was published in 1681, and would seem to be very rare; there is no copy of it either in the Library of the Royal Society or of the R. A. S. It consists, in fact, of two parts; copies of both are in the British Museum Library: but the former (the title of which is "Neuer Comet-stern welcher im November des 1680sten Jahres erschienen und zu Plauen im Voigtlande dargestalt observiret worden") is imperfect, the last (eighth, but not numbered) page ending in the middle of a sentence about the comet of 1652, which was observed by Hevelius. The second part (which deduces the parabolic form of the comet's orbit) is entitled "Astronomische Betrachtung des grossen Cometen welcher im ausgehenden 1680, und angehenden 1681 Jahre höchstverwunderlich und entsetzlich erschienen." Neither of these pamphlets gives the author's name printed on the titlepage; but in each it appears in manuscript, spelt Doerffel in the first and Doerfel in the second, being preceded in the latter case by the initials G. S. printed.

What I especially wish to point out is that although Newton, in the third book of the 'Principia,' apparently collected all the observations he could find of this comet (including some made in America), no notice is taken by him of those of Dörfel, so that we must conclude that he had not heard of them. He mentions Gottfried Kirch's at Coburg, the first of which was made on November 4 (O. S.); Dörfel states that the comet was seen by some unnamed person or persons at Leipzig on the 6th of that month. The perihelion passage took place on the 18th of December. Newton founds his determination of the orbit chiefly upon Flamsteed's Greenwich observations, which began on December 12. His own did not commence until 1681, February 25, and extended to March 9. Although the orbit determined by him is parabolic, he quotes with approval Halley's suggestion that it is really an elongated ellipse and the comet identical with those of B.C. 44, A.D. 531, and A.D. 1106. The probability now is that the period of the great comet of 1680–1 amounts to thousands of years, and that the comet of A.D. 531 was a return of that of 1682, subsequently known as Halley's, whilst those of B.C. 44 (seen shortly after the death of Julius Cæsar) and A.D. 1106 cannot be identified with any others. Yours faithfully,

Blackheath, 1899, June 23. W. T. LYNN.

The Meteoric Theory of the Gegenschein.

GENTLEMEN,—

As the phenomenon of Gegenschein has been frequently mentioned of late in your pages, a note relating to its explanation by the meteoric theory may perhaps interest some of your readers.

On this theory, the zodiacal light is sunlight reflected from minute solids circulating about the Sun, and thus forming part of the solar system. Since such solids, when in opposition, would appear as full moons to a terrestrial observer, it would seem at first that a maximum of reflected light should be seen on the meridian at midnight. But when the subject was mathematically investigated, it was found that this was by no means the case, and that, in fact, the discovery of Gegenschein tended to discredit the meteoric theory, instead of confirming it. This result was due to the circumstance that the investigators had employed the current theories with regerd to the effect of the phases of celestial bodies upon their brightness. In particular, Lambert's theory, developed in his 'Photometria,' had been regarded as almost demonstrably true, while, in fact, it consisted of an ingenious mathematical superstructure on a very insecure foundation.

Within the last fifteen years, the actual observation of the effect of the phases of planets, and especially of asteroids, has shown that the true law of phases is very different from that previously accepted; and on substituting these results of observation for those of the older theories in the expressions for the light to be expected from various directions, on the meteoric theory of the zodiacal light, it appears at once that there should be a slight maximum at opposition.

The existence of Gegenschein now becomes an important confirmation of the meteoric theory, and is so easily explained by that theory that previous hypotheses, of which there were many, have largely lost their interest. These previous hypotheses, by the way, are perpetually recurring in recent literature, after the manner of all such speculations.

A forthcoming volume of the 'Annals of Harvard College Observatory' will contain the results of many observations of Gegenschein, hitherto unpublished.

Cambridge, Mass.,
1899, June 19.

Very sincerely yours,
ARTHUR SEARLE.

Henry Ussher and the Dunsink Observatory.

GENTLEMEN,—

The founder of the Dublin (Dunsink) Observatory, like that of the Cambridge, died not long after he had got the establishment into working order, one consequence of which is that the name of the former (who was not, like Woodhouse, the author of any separate works) is now but little known. It is pleasant, therefore, to find an account of Henry Ussher in the fifty-eighth volume of the 'Dictionary of National Biography.' Perhaps I may just point out an error therein, where it is stated that his scientific papers will be found in the first three volumes of the 'Transactions of the Royal Society,' which should be those of the Royal Irish Academy. Ussher's description of the Dunsink Observatory forms the very first paper in the latter, and was read before the Academy on June 13, 1785. It arose from a bequest of Dr. Andrews, Provost of Trinity College, Dublin, which, some time after his death in 1774, was liberally supplemented by a grant from the College funds. Dr. Ussher was nominated the first Andrews Professor of

Astronomy, and commissioned to go to England and order from Ramsden the best instruments, without limitation of price. The transit-instrument was supplied without much delay, but the great eight-foot circle (originally intended to be ten) was not completed until many years* after the death of Ussher, which took place in 1790, and the appointment of his successor, John Brinkley.

Systematic observations commenced under the latter, the first to whom the title "Royal Astronomer of Ireland" was given, and who vacated his post on becoming Bishop of Cloyne in 1826. Ussher, however, contributed several papers to the 'Transactions of the Royal Irish Academy,' which will be found in the first two volumes. It is somewhat remarkable that the last two of these, "Observations of the Solar Eclipse of June 3, 1788"† (the same which was observed by Piazzi, with Maskelyne, at Greenwich), and "An Account of an Aurora Borealis seen in Full Sunshine," seem to have been added after the volume was supposed to be completed, and do not appear in its table of contents; in the British Museum copy they are omitted. Perhaps I may mention, with regard to the phenomenon described in the last, that I have on more than one occasion (but I cannot specify the dates) noticed what I could not doubt was produced by an aurora in the daytime, a radiation of streaks of clouds from the magnetic pole.

Yours faithfully,

Blackheath, 1899, July 3. W. T. LYNN.

The Thirtieth of February.

GENTLEMEN,—

On page 323 of the 'Observatory' for last year, your contributor, "From an Oxford Note-Book," refers to an inscription in Adderbury Church, giving the date of the death of a certain Jane Smyth as the 30th of February, 1508; and a communication from Dr. Geelmuyden, of Christiania, on page 422, informs us that, in Sweden, February had a thirtieth day in the year 1712. A perfectly clear explanation of the fact is also given. Although the Doctor's interesting letter was suggested by the curious date above referred to, it is, of course, no answer to your contributor's question thereon.

A little while ago I came across a similar inscription to the one in Adderbury, though of a considerably later date. It may be seen on a tablet in Cartmell Church, North Lancashire, and is in memory of

WILLIAM,
Son of
Robert and Ann Myers, who
Died February 30th, 1762.

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* It was, in fact, erected in 1808. See Sir R. Ball's account of the Observatory in the 'Book of Trinity College, Dublin.'

† The morning of June 4.

Possibly there are other instances of the kind, which, if known, might afford some clue to the explanation of these curiosities in English dates.

St. Anne's on the Sea,
Lancashire,
1899, July 21.

Yours faithfully,
ROBERT KILLIP.

A Statesman's Astronomy.

GENTLEMEN,—

Mr. Chambers has probably not been in the southern hemisphere or he would know what is meant by the "False Cross." I remember, when going out to the Cape a few years ago, the captain of the steamer pointed out to me the "False Cross" as a much more perfect cross than the "Southern Cross." It is formed by the four stars β, ϵ, and ι Carinæ, and δ Velæ. But if Mr. Chambers will refer to the 12th map in Proctor's Atlas, he will easily identify it.

32 Tavistock Square, W.C.,
1899, July 4.

Yours faithfully,
E. B. KNOBEL.

GENTLEMEN,—

With reference to Mr. Chambers's letter in the 'Observatory,' I believe the term "False Cross" has been applied to the constellation Corvus, which somewhat resembles the Southern Cross and has been frequently mistaken for it by people who are not familiar with the heavens.

Yours faithfully,
J. E. GORE.

Dublin, 1899, July 10.

OBSERVATORIES.

UNIVERSITY OBSERVATORY, CAMBRIDGE.—Reports for the twelve months ending respectively on May 25 and May 19 last have been presented to the Observatory Syndicate by Sir Robert Ball and Mr. Newall. From the former we learn that the observing done with the meridian circle during the year is summed up by the statement that 3616 observations of both Right Ascension and Declination of stars were made on 120 nights, with 65 determinations of Nadir Point, 27 of Collimation, and 28 of Level. The numbers of observations for adjustment seem rather scanty compared with the number of nights. Of these star-observations 176, made at the request of Col. Tupman, were of stars which have been observed at Harrow at occultation by the Moon. Others were Heliometer Comparison stars observed at the request of Dr. Gill; but the greater number of stars observed were those which occur in the Cambridge Zone Catalogue, but had not been observed more than twice. The reduction is well advanced, and an interesting investigation has been made of the probable error of an observation of transit over a single wire, which is found to be $0^s \cdot 0491$.

An account of the new telescope, which is to be known as the Sheepshanks equatorial, has been already published (Mon. Not. R.A.S. 1899, Jan.). Sir Robert Ball makes note of several of the difficulties which have been encountered, such as always are in the setting up of a new instrument, and have been successfully overcome by Mr. Hinks, who has charge of the instrument. A machine for measuring photographs is being constructed for the Observatory, for use in stellar parallax work, on a plan which contains a small modification of that which obtains in the Greenwich instruments: the scale in the eyepiece has a small movement by micrometer-screws, so that the measurements are made in terms of the whole divisions of the scale and in parts of a division, which are measured by the screws. The number of members of the University attending the classes in Practical Astronomy shows an encouraging increase.

Mr. Bryan Cookson, a research student in Astronomy, has been engaged in preparing for an investigation of the constant of aberration by a floating photographic zenith telescope. The small dome on the top of the building has been placed at his disposal.

Mr. Newall reports a continuation of the work mentioned last year, the taking of photographs of stellar spectra with the Bruce spectroscope for the determination of velocity in the line of sight. During the year 150 photographs, besides others rejected for various reasons, have been obtained.

Preparations are well advanced for converting the Bruce spectroscope into a powerful four-prism instrument, which is to be used in securing material for a detailed examination of the spectra of a few of the brightest stars.

Mr. Newall is making an exhaustive discussion of the valuable photographs of the spectrum of the Sun's limb which he obtained in India on the occasion of the eclipse in 1898, January. He appears to be comparing this spectrum with that of certain stars, and also with that of certain terrestrial elements. We shall probably hear more of this later.

YALE.—Dr. Elkin's Report for the twelve months ending 1899 June 12 is brief enough to quote almost in full:—"The heliometer has been employed during the past year in securing the final measures for the parallax series on stars of large proper motion, which has thus been brought to a close. The reduction of the results is also approaching completion and will probably be available in the near future. Dr. Chase has also continued the investigation on the refraction of highly coloured red stars in order to strengthen the results already obtained. These appear to show that systematic error from this source is hardly to be feared with the heliometer, and hence further discussion of our parallax results to be scarcely requisite.

"With the aid of the grant from the J. Lawrence Smith fund of the National Academy, a second station for photographic observation of meteors was put up in Hamden, about two miles in a northerly direction from the Observatory. The apparatus used was a simple form of equatorial mounting without clockwork, and carried four cameras with portrait-lenses of about five inches aperture. Dr. Chase took charge of this station, and it was first put into operation at the August meteor period of last year. Owing to moonlight and the generally hazy and damp atmosphere, only one meteor-trail was secured, which was, however, not a Perseid, while at the Observatory station only this meteor and one other, a Perseid, were recorded.

"At the epoch of the Leonid shower, which is now near its maximum, watch was kept throughout the nights of Nov. 12 to 16, and complete records obtained on the two nights, Nov. 14 and 15. On these in all 16 meteor-trails were impressed on the plates at the two stations, 10 at the Observatory, and 6 at Hamden, 8 of which were Leonids. Four meteors were recorded at both stations, but unfortunately only one of these was a Leonid. An interesting bye-product of the work was the discovery on examining the plates of Nov. 21 of a faint comet by Dr. Chase. The Geminid shower was quite pronounced on Dec. 11, and two meteors from this radiant were secured at both stations besides a fifth trail at the Observatory."

PUBLICATIONS.

THE INDIAN ECLIPSE, 1898 *.—A total solar eclipse is like the measles—it leaves such terrible after-effects. Since January 1898 the luckless reviewer has had, about once every two months, to raise himself to a condition of enthusiasm over a Report by somebody of the eclipse which was so excellently observed from India about a year and a half ago. This time the task is comparatively easy, for the Report of the British Astronomical Association just published, edited by Mr. E. W. Maunder, is most cheerful reading, and is illustrated by a host of interesting pictures which require little effort of mind to understand. It will be remembered that the expedition, organized, as Mr. Maunder points out, on the principle laid down by the Pickwick Club, whereby everybody paid his own expenses, consisted of several persons, mostly amateurs, who went to one of two camps, at Talni and Buxar, and there made whatever observations each thought fit. The book now before us is made up of general accounts of the camps at the two places (by Mr. Maunder and Mr. Bacon), and also of the camp at Jeur by Mr. Cousens, where various parties other than those connected with the Association were stationed, and of Reports by individual members of their own observations, with a chapter about some places of interest visited after the eclipse, and a final summary of results. Without wishing to be invidious, it appears that Mr. Evershed's is the most scientific contribution, and Mr. Maunder's the most readable. In this last there is a specially interesting photograph of the Sun's images as thrown on a pavement through interstices in a tree during the eclipse. It is a matter of common knowledge that these images are usually circular or of oval shape—here they are of crescent form. Mr. Wesley contributes a characteristic page or two on the shape of the corona as seen at this eclipse, the main parts of which have already appeared in these pages. The summary of results obtained, on which the Association is certainly to be congratulated, is given as follows:—

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* 'Report of the Expeditions organized by the British Astronomical Association to observe the Total Solar Eclipse of 1898 January 22.' Hazell, Watson, and Viney, London, 1899. Price 5s.

1. The spectrum of the "Flash" has been successfully photographed on five plates.
2. A new region of the "Flash" spectrum has been explored and mapped.
3. The bright-line spectrum of hydrogen has been followed in the chromosphere to the 30th point of the series, and the wave-lengths shown to agree closely with Balmer's formula.
4. The correction to the received wave-length of the "coronium" line detected by Mr. Fowler has been confirmed.
5. The distribution of "coronium" in the corona has been examined.
6. Several drawings of the corona as seen by the unaided eye have been made.
7. The variation of the form of the corona with the sun-spot cycle has been further illustrated.
8. Twenty-eight photographs of the corona have been taken.
9. These include several showing the coronal rays to a greater distance from the Sun than they have ever been photographed before.
10. And cast fresh light upon their structure.
11. Besides restoring the credit of many old drawings of eclipses which had been supposed untrustworthy because of their want of resemblance to the photographs.
12. The wide range of exposures employed furnishes a most valuable guide as to the exposures to be employed in future eclipses.
13. The corona has been definitely photographed during the partial phase.
14. In several different and independent ways a measure has been obtained of the total illumination given by the corona.

For description of the way that all these results were obtained, and for hints how similar valuable results may be obtained at a future eclipse, we must refer our readers to the book itself. We can promise them that their five shillings will be well laid out.

Rousdon Observations: Variable Star Notes, No. 5.—It may be remembered that at Sir Cuthbert Peek's Observatory at Rousdon, Devon, the staple work is the observation of well-known variable stars, and that he publishes, at intervals, handy little books which give the actual observations made by himself or his assistant, Mr. Grover, of (generally) two such stars, extending over a long period, with tabulation of these results in the form of curves. The book just published gives the observations of U Orionis and S Herculis for the thirteen years 1886–1898. It might add to the interest of these if a discussion were given leading to a formula which would give the period and limiting magnitudes. Perhaps this will come later; meanwhile the clear way in which the results are presented permits this to be readily done by any who wish for such information.

NOTES.

Comet Notes.—Ast. Journ. 465 has a note by Mr. Perrine on the re-discovery of Holmes's Comet on June 10. It was found with the 36-inch refractor, being a round nebulous mass 30″ in diameter, slightly brighter in centre. It was very faint (about 16th mag.) and difficult to observe, though the sky was quite clear.

The following is an ephemeris for Greenwich noon (Ast. Nach. 3582):—

	R.A.			N. Dec.	
	h	m	s	°	′
Aug. 4....	2	36	42	33	4
8....	2	41	31	34	9
12....	2	46	3	35	11
Aug. 16....	2	50	17	36	15
20....	2	54	11	37	16
24....	2	57	44	38	17

Swift's Comet had become very faint by the end of June, though it still remained well defined and permitted accurate observations to be made. An ephemeris up to August 7 was given last month, but the comet is probably now too faint for ordinary telescopes.

A. C. D. C.

Minor Planet Notes.—A new planet, EO, of the 11th magnitude, was discovered by Wolf on July 17. A. C. D. C.

On the Effect of Temperature on the Scale-value of a Telescope-Micrometer. — In No. 462 of the 'Astronomical Journal,' Prof. Barnard publishes a remarkably fine series of measures of Neptune's satellite, made with the 40-inch Yerkes refractor, which, taken in conjunction with those made in 1897–98, show the satellite's position on 103 nights in the two seasons.

The notes accompanying these measures would be spoiled by abbreviation; they are therefore quoted here in full:—

"The mechanical portion of the telescope has performed well during the extremely cold weather. The clock served well down to a temperature of 12° or 15° below zero, when it slowed up. In these cold temperatures the telescope worked as easily as in the warmer, and the micrometer was perfect.

"The canvas curtain across the observing slit was of great service.

"The great telescope seems remarkably stable in its several parts. The object-glass changes figure in the wide range of temperature to which it has been subjected. This is shown by the measures of the difference in declination between Atlas and Pleione of the Pleiades, which were made in temperatures as low as 22° below zero (F.), and as high as +80° (F.), *i. e.* a range of 102°."

During the observations the temperature and scale-readings of the focussing-tube were recorded, and below is a brief summary of the results. As these changes may not repeat themselves exactly when under identical conditions, it is intended to continue the measures during another season.

	Mean Temp. (F.).	Mean scale-reading.	$\Delta\delta$.	Number of nights.
	°	inches.	″	
1898 July 26–Sept. 3	+72·81	2·269	0·676	10
1899 Jan. 28–Feb. 12	−10·17	2·011	0·491	9
	82·98	0·258	0·185	

(31 complete revolutions of the screw are omitted in $\Delta\delta$.)

From these it will be seen that

(1) Focus of O.G. shortened 0·26 inch more than did the tube itself.

(2) The separation of stars lessened nearly 0″·2.

"Assuming a uniform value for the revolution of the micrometer-screw (9″·677), the shortening of focus would make the measures less in winter, while the contraction of the micrometer-screw would make them larger. The steel tube would shorten about 0·44 inch, and hence the total shortening of focus of the 40-inch would be 0·70 inch, producing a change of about 0″·27 in the measures. The contraction of the screw would produce an increase in distance of about 0″·17, which, taken from 0″·27, leaves 0″·10 as the lessening of the distance-measures of the two stars from shortening of focus and contraction of screw."

The difference between the summer and winter measures is thus accounted for to the extent of 0″·10, which leaves 0″·08 to be attributed to the error of observation.

It would seem, therefore, that a large glass in a severe climate such as this is subject to considerable change of form, which would interfere with very exact work, such as for parallax, if not strictly taken into account. It is probable, also, that the molecular change in the object-glass may prevent these changes from repeating themselves under the same conditions of temperature. I am rather led to this belief by the measures of Atlas and Pleione during the fall and winter of 1897. At that time the measures were carried over the interval from 1897 August 27 to 1898 January 1, through a range of 49° of temperature, and it was clearly shown that the apparent distance between the two stars increased to the extent of 0″·2 or 0″·3 as the cold weather came on.

Careful determinations of the colour-curve of the great object-glass have been made by Prof. Frost and Mr. Ellerman during wide extremes of temperature, and are now being investigated.

ON THE PRESENCE OF OXYGEN IN THE ATMOSPHERES OF CERTAIN FIXED STARS.—For the following abstract of a paper communicated to the Royal Society by Dr. Gill we are indebted to 'Nature' of June 22:—

> "The observations described confirm the conclusions arrived at by Mr. F. McClean and Sir Norman Lockyer as to the existence of oxygen lines in the spectrum of β Crucis. From measures of photographs of the spectrum of this star, it is concluded that the whole of the known helium lines within the measured range of spectrum are unquestionably present, as also are all known oxygen lines stronger than intensity 4.
>
> "There remains not the slightest doubt that all the stronger oxygen lines are present in the spectrum of β Crucis, at least between *l* 4250 and 4575, and this fact requires no further laboratory experiments for its establishment. It is almost equally certain that there is no trace of true nitrogen lines in this spectrum.... Besides hydrogen, helium, and oxygen, the spectrum of β Crucis shows the probable presence of carbon (4267·2) and magnesium (4481·17).... The spectra of β Crucis, β and ϵ Canis, and probably β Centauri are all practically identical."

A SUGGESTED RELATION BETWEEN MAGNETISM AND ASTRONOMY.—We regret to think that we did a little injustice in the note on the Proceedings of the Toronto Society in our May number, by omitting to make mention of two subjects contained therein which certainly might be considered as original astronomical work. The first is an interesting account of a large meteor seen by several observers over the peninsula of Ontario on 1898, July 5. The discussion of the observations forms interesting reading. A general conclusion seems to be that it moved for about 200 miles at the rate of about 23 miles a second. The other matter was contained in Mr. Harvey's Presidential address. He claims to have discovered that while a magnetic storm is proceeding on the Earth, extraordinary phenomena are simultaneously taking place on other bodies of the solar system. Mr. Harvey gives as illustrations of this that Encke's comet changed its form, that the tail of Coggia's comet 1874 became exaggerated, that Brooks' comet 1893, and comet 1861 II., suffered a change, all on days when the magnetic records are abnormal. In a letter Mr. Harvey tells us that he has lately read a paper before the Royal Society of Canada, in which he gives a continuation of his researches and gives, besides, other instances of changes in comets,—instances of cases when aureoles were seen round Mercury and Venus when magnetic movements were excessive. We need scarcely add that there is nothing *proved* as yet, but the suggestion is commendable.

WHITE SPOT ON JUPITER.—Herr Phil Fauth announces in Ast. Nach. No. 3570 that he has several times seen with his telescope of 7 inches aperture a white spot on the north-eastern belt of the planet. The spot was seen to pass the central meridian on May 8^{d} 11^{h} 25^{m} and on May 18^{d} 9^{h} 33^{m}.

AN EXPEDITION FOR THE EXPLORATION OF THE ANTARCTIC REGIONS.—The Royal Society has combined with the Royal Geographical Society to organize a scientific expedition to the Antarctic. An application to H.M. Treasury has been favourably received, and the Government will contribute the sum of £45,000, to be spread over four years, of which the financial year 1900–1901 will be the first, towards the expenses.

In making this grant the authorities definitely state that this must not be taken as precedent. This is not to be taken as inaugurating a new era of more extensive grants to science.

MEASURES OF JUPITER'S SATELLITE V.—In Bulletin No. 10 of the Yerkes Observatory, Prof. Hale publishes the results of Prof. Barnard's latest measures of this satellite. Greenwich times of east elongation and distances corresponding are as follows:—

	h	m	″
1899 April 25	19	5·26	48·34
1899 May 1	18	32·72	48·29

As this satellite has made more than 4800 revolutions since the first observation of elongation in 1892 September, its period can be determined with great accuracy. Comparison of the observations of 1892 with those of 1898 and 1899 give as value of the period 11^{h} 57^{m} $22^{s}{\cdot}647$, which is thought to be in error by not more than $0^{s}{\cdot}01$. The observations were made with the 40-inch telescope, which, Prof. Hale points out, is evidently suitable for the measurement of faint and difficult objects.

ON Thursday, July 27, the Astronomer Royal received the members of the Spectacle-Makers' Company, to the number of about 150, to inspect the buildings and instruments of the Royal Observatory. The livery-dinner of the Company was afterwards held at the Ship Hotel, Greenwich, to which the members of the staff were invited. Sir Robert Ball and Mr. E. B. Knobel were present as Spectacle-Makers.

As a consequence of the retirement of Sir John Donnelly from the Secretaryship of the Science and Art Department, Capt. Abney has been appointed principal Assistant Secretary.

LORD KELVIN has retired from the Professorship of Natural Philosophy in the University of Glasgow, which he has held for fifty-three years.

IT is announced in 'Science' (America) that a Board of Visitors, consisting of the following members, has been appointed to report upon the state of the U.S. Naval Observatory:—S. C. Chandler, A. G. Drayton, G. C. Comstock, G. E. Hale, and E. C. Pickering.

THE same journal states that Dr. Milton Updegraff, Professor of Astronomy in Missouri University, has been appointed Professor of Mathematics in the U.S. Naval Observatory.

THE Magnetic Observatory at Vienna has had to be disestablished owing to the increase of electric tramways and electric-light wires in its neighbourhood. A new observatory is to be established at some distance from the city.

FROM AN OXFORD NOTE-BOOK.

WE have been accustomed to associate the name of Schiaparelli with observational work of the highest class, especially the detection of faint or minute planetary detail; but a recent memoir* brings the distinguished astronomer before us in a new light as a philosophical historian. It is known that 50 or 60 years after Aristotle had propounded a geocentric theory of the universe (330 B.C.) the Copernican system was announced by Aristarchus of Samos, and was accepted as a probable hypothesis (though it was afterwards abandoned). What causes led to this rapid evolution of ideas? This is the question to which Schiaparelli attempts an answer; and the answer he gives (if my cursory glance through the paper has not misled me) is that the Greek physicists were led as a direct result of observation to look for material centres for all circular motions. The idea of a subsidiary rotation of certain bodies round a centre different from that about which the universe resolved arose first of all in connection with the phases of Mercury and Venus, whose variations in brightness suggested to Heraclides Ponticus to attribute to them circulation round the Sun. The same idea suited the observations of Mars, and, on further trial, of Jupiter and Saturn; and thus was evolved the Tychonian system, with Sun and Moon revolving round the Earth, and the five planets round the Sun. The step to the Copernican system was obvious, and was definitely taken by Aristarchus of Samos, though foreshadowed by Heraclides himself. But all this time there were no epicycles without material centres. When the mathematicians found the elliptic inequalities explained by epicycles without material centres, they at once abandoned the system of Aristarchus, which had always been felt objectionable in attributing motion to the Earth, and thus undermining astrology.

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* "Origine del sistema planetario eliocentrico, presso i Greci." Mem. del R. Istit. Lombardo di Sci. e Lett. vol. xviii. fasc. v. (1898).

The point made by our illustrious planetary observer of the nineteenth century, that the epicyclical idea was arrived at by his colleagues of 2200 years ago as a direct result of observation, is most attractive. If the evidence stands the test of thorough examination, it will always be pleasant to remember that Schiaparelli first drew attention to it. My cautious "if" is due to the remarks of an Oxford friend, who knows something of such matters, and with whom I have (very inadequately) discussed Schiaparelli's memoir. He is rather sceptical about the explanation offered. He says the Greeks thought circular motion as natural and elementary as motion in a straight line, and would never have felt any necessity for a material centre. Such points are for others to decide; but I can heartily recommend any astronomer to read the memoir.

THE *Daily Mail* is showing a praiseworthy interest in the big telescope for the Paris Exposition next year; but I fear the variety of its information on different dates may confuse its readers. On November 2 last it gave a creditable approximation to the proper design—mirror, horizontal telescope, and screen were all represented, though the clockwork for the mirror was not shown; on the screen was a rather impossible mountain in the Moon; and the instrument was "to bring the Moon within two miles." But on March 30 a quite different picture was given, showing a large equatorial of the ordinary pattern, with a small Eiffel Tower as pier. Letterpress with the picture stated that the magnifying-power was to be 6000—rather a drop from the 120,000 of November 2. The length of the whole instrument is reduced from 197 feet to 185 feet; but even then one cannot deny the truth of the comments that "a man beside it will look a very small and insignificant object," and "its great length will reach high into the air." But by April 26 new information is to hand; no reference is made to the story of March 30; indeed, since it is now remarked that "a telescope of such proportions would fall down of its own weight if constructed on the ordinary upright principle," it would seem wiser to omit any reference to the previous information, though, as above remarked, readers of both articles are likely to be confused a little. The design of April 26 is the correct one, being copied (without acknowledgment) from a French authority; or possibly from the *English Mechanic* of April 14.

The occasion of the Stokes Jubilee, to which I referred last month, brought many distinguished visitors to England, among them Simon Newcomb, and Oxford honoured itself by giving him an Honorary Degree (D.C.L.) at such a favourable opportunity. On such occasions the Professor of Civil Law recites briefly the reasons for the honour; and it was interesting to note that though Prof. Newcomb's work has been so vast and varied, it is still capable of a concise description, in that the ultimate aim of nearly all has been the construction of new tables of the Sun, Moon, and planets. As a contrast to the brevity and business-like character of this summary of a life-work I strongly recommend those who have not seen them to read the "Reminiscences of an Astronomer" in the *Atlantic Monthly* for August, September, and October, 1898, wherein Prof. Newcomb gives a delightful collection of odds and ends. I wish this Magazine or another would republish them, so that astronomers generally might have them accessible. Perhaps I may tempt some editor by giving two specimens:—

> When with Argelander as a student, Gould was beardless but had a good head of hair. Returning some years later, he had become bald, but had made up for it by having a full, long beard. He entered Argelander's study unannounced. At first the astronomer did not recognize him.
>
> "Do you not know me, Herr Professor?" The astronomer looked more closely. "Mein Gott! it is Gould mit his hair struck through."
>
> I believe (Adams) was justly credited with an elaborate witticism to the following effect:—"In view of the fact that the only human being ever known to have been killed by a meteorite was a monk, we may concede that after 400 years the Pope's bull against the comet has been justified by the discovery that comets are made up of meteors."

Dr. Gill's report for 1898, just received, tells us a number of interesting things, and not the least important is that the telescope presented to the Cape Observatory by Mr. McClean has been received and set up, and we may confidently expect a long spell of fine work from it. But there are apparently thorns even in the bed of roses which a man might fairly expect to lie on after making such a generous gift to astronomy. On the receipt of the telescope

> It was found that the stand required considerable modification, and Mr. McClean most generously authorized the carrying out of all necessary

alterations. The requisite iron castings and stays were designed, made, and fitted at the Cape, and the stand is now in every respect most steady, satisfactory, and convenient. The fittings for electrical illumination of the circles, scales, and micrometers, including the switches, rheostats, and fuses, had to be made or remodelled at the Cape.

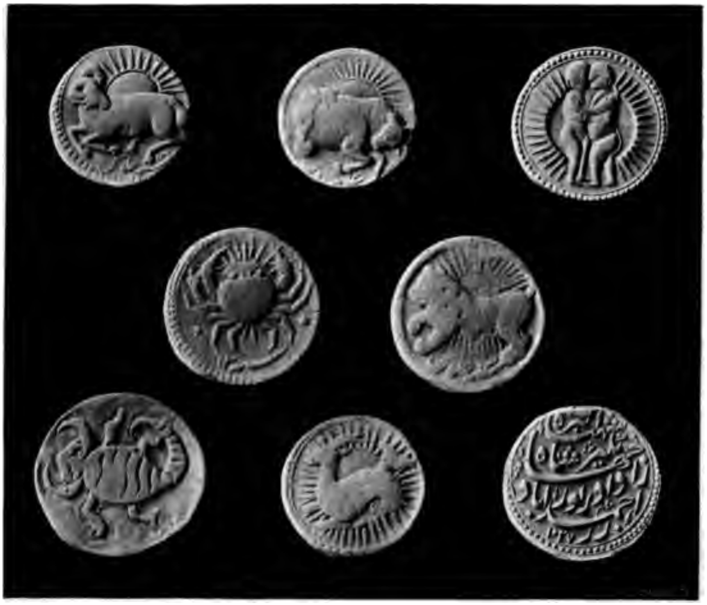
* * * * * *

The 24-inch photographic object-glass has unfortunately two faults—the marginal images show well-marked *coma*, and the minimum focus, instead of being for rays of the refrangibility of $H\gamma$ (or, if anything, on the violet side of $H\gamma$), is for rays of refrangibility about midway between $H\beta$ and $H\gamma$. **It is understood that Sir Howard Grubb will remedy these defects.**

It looks as though the instrument-maker had not been very careful with the telescope. Surely in the case of a gift of this kind it would have been gracious in him to have expended a little more care than usual, rather than less?

It has been incidentally remarked before in these Notes that the Professorship of Astronomy at Oxford carries with it a Fellowship at New College, and the corresponding College duties. These have involved, both last year and this, an official visit to the College property. The ceremony is called a "Progress," and the people taking part in it are the Warden (or Subwarden representing him), the Outrider (one of the Fellows selected to represent them), and the Steward (the legal adviser of the College). In old days the Progress collected in hard cash the revenues of the College, and, indeed, until quite recently, considerable sums were paid directly to them. The present Warden, for instance, can remember finding himself (though before he was Warden, and when filling the office of Outrider) with more than £1000 in bullion, and he remembers also that he had the discretion to promptly pay it into the Aylesbury Bank. I cannot indicate the date of this occasion more definitely than by remarking that the Warden is at present a vigorous gentleman of 89 summers.

This brief explanation is given in order that I may record the following fact. The Progress this year extended to some portions of the College property which had not been so visited for eleven years, and on turning up the name of the Outrider who had acted on the last occasion, we found it to be that of Milner; and I take some pride in drawing attention to this fact, that Sir Alfred Milner, on whom a good deal of attention is just now fixed, is an actual Fellow of New College (and might draw revenues from it if he chose), and 11 years ago filled the office which the writer of these notes had the honour of filling last year. I will not indicate what may therefore be fairly expected of the aforesaid writer in ten years' time. I leave that to be inferred. But it seems well to take advantage of this opportunity to draw an interesting parallel, in case an increase of modesty might prevent my drawing it later.



INDIAN COINS.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXII. SEPTEMBER, 1899. No. 283.

Catalogue of Fundamental Stars for 1875 and 1900 *.

The words fundamental and absolute are terms of serious import, but nevertheless of an elastic nature. They are meant to imply that the positions of the stars in this catalogue are about as perfect as science can make them from available material. The imperfections of the older catalogues, personal equations depending on magnitude, personalities in the observations of the Sun for the determination of the equinox correction, and the effect of periodic variation of latitude on measures of absolute declination, all tend to frustrate the most refined discussions from attaining to perfection. Doubtless a system such as the present one, and that formed by Prof. Auwers, is the best method for keeping in line and for extracting the most efficient results from the numerous sets of observations that are continually being turned out from various observatories in different parts of the globe. Though it is well known that the fundamental catalogue formed by Auwers some years ago, and upon which is based that most excellent series of observations generally termed the "Astronomische Gesellschaft Catalogues," requires certain corrections both in right ascensions and declinations, yet the value to astronomy from the formation of a system which has turned chaos and disorder into an instrument of orderly research and unity of aim and purpose is incalculable. The tables so laboriously formed by Prof. Auwers by comparing every catalogue of note with his fundamental positions form the basis of the corrections which destroy his first system and, Phœnix-like, become the progenitors of a new and better one. The differences which arise from a general comparison of different star-catalogues with definitive positions for a given epoch may be generally classed under two headings in case of each co-ordinate, one depending solely on right ascensions and the other on declinations: the one including all stars of whatever declination which have the same right ascension, the other all stars of all right ascensions which have the same declination. These differences may be divided again into two classes—those which arise from varying data of reduction, and which can be easily determined and allowed for, and those which are found from comparing two catalogues together after all the data of reduction have been made to accord, and which are generally known as systematic. Were the observations all made at the same epoch, these discrepancies would be found by a simple comparison; as, however, observations are made at different epochs, a direct comparison can only be made by reducing the observed portions to a common epoch by precession and proper motion. The motion of the equator and equinox through precession being taken as known, the proper motion of each star is the unknown quantity of the whole problem which has to be solved. Hence the correction to be applied to the different authorities to reduce them to one and the same general mean will be represented by a correction of the form $x+y\mathrm{T}$, where x will represent the mean correction to the co-ordinate of the star at the time $\mathrm{T}=0$, and y will represent the variation of this correction. If the weights of the several catalogue determinations have the same ratio in the case of the different stars, the computed position of the star will be the same whether the systematic errors are ignored or corrected; the advantage of this method therefore only appears when the accidental and systematic errors are not in the same ratio in the case of different authorities.

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* Catalogue of Fundamental Stars for 1875 and 1900 reduced to an absolute system. By Simon Newcomb. (Astronomical Papers prepared for the use of the American Ephemeris and Nautical Almanac, vol. viii. part ii.)

Prof. Newcomb in order to secure comparisons at different epochs between the different star-positions has adopted, in the case of the right ascensions, "The right ascension and annual variations of the standard stars found in the Catalogue of 1098 clock and zodiacal stars" published in pt. iv. vol. i. of the Astronomical Papers prepared for the use of the 'American Ephemeris and Nautical Almanac' (this is the system N_1 adopted by the Paris Conference) as a provisional standard. The place of any star for any epoch having been computed from these data, the comparison between the computed and observed places gives data for finding the correction for equinox, and a correction depending on right ascension in the case of those catalogues supposed to need such a correction. For corrections to the right ascension depending upon declination Auwers's tables of corrections to various catalogues has been used. In the case of the declinations as a provisional standard, Boss's well-known catalogue of standard declinations of 500 stars, found in the report of the Northern Boundary Survey, has been similarly used. As a result of his present study, Prof. Newcomb states that he has come to the important conclusion that the practice of corrections for systematic errors has been in present times carried too far. All corrections for seemingly systematic errors which cannot be plausibly traced to a known cause should be regarded with great suspicion; in fact actual harm may be done to the results by regarding a difference as systematic which is only accidental, but to lay down a limit within which such a difference may be disregarded is well nigh a practical impossibility.

It is well known that when observed declinations, even with the best instruments, are compared, systematic deviations appear to which it is difficult to assign a cause. Probably they may be due to a variety of causes, such as unknown flexure, systematic error of graduation of the circles, deviation of the latter from a plane, and personal equation of the observer. In any case there is no certainty that such errors are eliminated in the general mean, and an independent normal test is a very valuable system of check. Such a standard Prof. Newcomb has set up, on the principle that the motions of the Earth and planets around the Sun take place in a plane or, when projected on the celestial sphere, in great circles. In working up the theory of all the planets all observations of declination were reduced as far as possible to the Boss system. It therefore follows, if δ be the mean error of this system within the zodiacal limits, then all the observations thus reduced will need the common correction δ in order that the motions derived from them may take place on a great circle of the sphere. This process only gives the mean error through the entire zone of declination included within the zodiacal limits and around the whole circle of right ascension, but affords no clue to the possible variation of the error either from zone to zone or from hour to hour. For such variations each instrument must supply its own data, and in general it has been assumed that such a correction will vanish at the poles. A noteworthy feature of the results is the agreement between the best authorities as to the amount of the correction in the different zones, so much so, indeed, that Prof. Newcomb appears to be somewhat anxious as to whether the absolute errors of our declinations can really be as small as the discordances in question would indicate.

In dealing with the errors of right ascensions, little difficulty is experienced in the cases of stars near the equator. The usual practice of fixing all right ascensions by the determination of clock-errors derived from clock-stars makes the determination of the systematic corrections comparatively easy. It is only when the stars are such a distance from the equator that the error of right ascension depending on the declination may become considerable that difficulties begin to appear, and naturally increase as the pole is approached, near which the derived right ascensions depend mainly on the polar azimuth. This is an instrumental quantity which can only be rigorously derived by consecutive transits of the same star above and below pole, and which presupposes such a stability in the instrument that no serious change in the quantity may be likely to take place in the interval. Such observations are only made habitually at a few of the standard observatories. In other cases the quantity is derived from a comparison with adopted right ascensions of polar stars, in which case it is evident the mean error of the adopted places will be reproduced in all the observed right ascensions near the pole. A discussion of the four close polars reveals a fairly satisfactory agreement between the various authorities, and Prof. Newcomb has come to the conclusion that the precision of the results should be beyond the needs of astronomy in the immediate future. Systematic differences between right ascensions determined with different instruments and varying with the declination may arise from several causes. If the pivots of an instrument are not truly cylindrical, the line of collimation will not describe a great circle on the celestial sphere. If an observer's personal equation differs for fast- and slow-moving stars, then the right ascensions at considerable distances from the equator, and especially near the pole, may suffer considerably; again, at the zenith an observer reverses his position relative to the diurnal motion, and this may cause a change in his personal equation. Again, if an observer's personal equation varies with magnitude, then we should expect a gradual change in the observed right ascensions dependent on the absorption of light by the atmosphere, and which will only become apparent by comparing observations made at a northern and southern observatory.

It may be interesting to state that the only correction Prof. Newcomb finds it necessary to apply to Auwers's reduction of Bradley's right ascensions is a constant of $-0^{\mathrm{s}}{\cdot}079$ for equinox, a quantity which may be affected by the fact that the Bradley right ascensions enter into the original system N, and owing to the distance of the date must have materially influenced the proper motion of that system, notwithstanding the smallness of the weight which was given to them. A somewhat larger quantity would have better suited more modern observations of the Sun, and tended to diminish the discrepancy between the precessional motions derived from observations in right ascension and declination. The corrections found to the Bradley declinations are considerable. It may be remarked that for the first three years Bradley's quadrant was turned towards the north and used exclusively on stars culminating between the north horizon and two or three degrees south of the zenith; the exact period was from 1750, August 21, to 1753, July 26. It was then turned towards the south, and from 1753, August 5, until 1762, July 16, was used in observing southern stars. Thus the north and south stars, as they may be called, form two separate classes, to which a zone about six degrees in breadth, including the zenith, is common. It was probably through the common stars within this zone that Auwers was enabled to form a complete system of the whole.

The corrections to the various catalogue-positions of a star to reduce them to the normal system are transformed by simply changing the sign into the correction to the provisionally normal systems as given by each catalogue and, with suitable weights, form the bases of the new definitive system of right ascensions and declinations finally adopted by Prof. Newcomb for the epoch 1875·0.

The question of the personal equation of an observer varying with the magnitude of the star is a subject of considerable discussion by Prof. Auwers, the results of which are that for each change of magnitude the difference on the average amounts to 0^s·01, and is the same for both the eye and ear and chronographic method of observation. These values depend finally on the values deduced by diaphragming the objective, a method which may diminish theoretically the intrinsic light of the object, but which, according to the writer's experience, appears very generally to increase the apparent light area, and reduces an ordinary star to a more or less state of nebulosity. One can easily understand that there should be a personality in observing bright and faint objects, the faint objects being observed later than the bright ones, because a faint star will not bear so much illumination in the field, and consequently the spider-lines are not so distinct, and it therefore becomes more difficult to tell the exact moment when the spider-line appears to bisect the star-image. It, however, seems remarkably strange that such a personality should develop itself symmetrically over the range of star-magnitudes; one would rather expect that it would develop quickly with reference to the fainter stars. That the source of error is a reality seems to be amply proved by the results of heliometer observations; the difficulty appears to be how to find a satisfactory method of determining its value. The results in Prof. Newcomb's catalogue have not been corrected for this apparent source of error.

The catalogue contains 1596 stars distributed over the 24 hours and reaching from pole to pole. The star portion has been reduced to 1875 as the fundamental epoch, and brought up to 1900 for the convenience of astronomers. The precessions employed are those derived by Prof. Newcomb according to the directions of the Paris conference, and the proper motions accord therewith. The equinox of the catalogue is that of the system N_1. The right ascensions have not been corrected for personal equation depending on magnitude. The declinations depend on Poulkova refractions, and have not been corrected for latitude variation. The system of declination has been constructed on the principle that the motions of the planets around the Sun take place in a plane which, as seen from the Sun, will be projected as a great circle of the sphere. The correction thus adopted as applied to Boss's equatoreal declinations amounts to $+0''·09+0''·38$ T. At the end of the catalogue are given formulæ and tables for the reduction of mean places of the fixed stars.

It seems superfluous to add that astronomers are placed under fresh obligations to Prof. Newcomb for the valuable data contained in this volume. W. G. THACKERAY.

Six Months' Work with a Seismograph at the Royal Alfred Observatory, Mauritius.

II.

Prof. Milne originally accounted for the diurnal changes in level by the different evaporation effects on the east and west sides of the instrument.

Writing to the 'Geographical Journal' he says:—"The general conclusions to which the various observations point, is that the movements which take place during the day are due to the removal of a load from the side of the station most exposed to the effects of radiation, and in the alluvium this effect is quite pronounced to a depth of at least twelve feet. This load may be represented by aqueous vapour carried upward and then dissipated.

"On a fine day, experiment shows that from moist open ground as much as one ton of moisture may be removed from an area 20 yards square.

"If an action of this sort is more marked upon one side of an instrument than upon another, then the ground by its resilience rises on the former side and the pendulum swings away from the area which has been most relieved ..... From the researches of S. H. Miller it would appear that the average daily quantities of evaporation for *particular* months are approximately as follows:—

"Soil (humus; July)	4·243	lbs. per sq. yd.
Water (July 1869)	8·613	" "
Forest (a spruce)	12·528	" "
Grass (red clover; May)	15·613	" "

These loads, which are removed during twelve hours of daylight, may be either greater or less than the quantities given; they are practically in the ratio of 1 : 2 : 3 : 4.

The greatest daylight displacement of a horizontal pendulum ought therefore to be expected when such an instrument was placed on the boundaries of two areas respectively covered with soil and grass. On a fine day the differential evaporation effect on the two sides of the instrument would be equal to removing a load of about 2½ tons per 20 yards square from the ground covered with grass, which is quite sufficient to produce many of the observed effects."

The following year Prof. Milne states that experiments made with tarpaulin placed first on one side of the tent in which the seismograph was placed, and then on the other side, showed that the diurnal motion was not only uninfluenced by differential evaporation effects, but also by the heat received by the ground on the two sides of an installation.

From experiments made with instruments registering under different conditions, Prof. Milne also concludes that the diurnal motion is not caused by warpings of the pier or portions of the instrument; having as the alternative "external effects on a widespread area," as represented by a solar "land-tide," for instance.

Prof. Milne's theory of differential evaporation effects would then be somewhat as follows:—

Just as the Moon heaps up an ocean-tide towards itself by the force of gravity, so the Sun by radiation (and consequent release of strain by removal of aqueous vapour) heaps up the Earth, and this "land-tide" travels round the Earth in the same manner as the ocean tide, and, as it approaches a certain point, travelling from east to west it will create an upheaval to the east of that point which will throw the seismograph boom towards the west until the highest point has passed; the boom will then gradually settle down again, and finally return to its original position.

But seeing that instead of moving to the west from a little after sunrise till the afternoon, it moves to the east, and bearing in mind the behaviour of the boom on December 5, 6, and 7, we are led to the conclusion that this supposed "land-tide" is in the form of a depression from contraction and sinking, and not an upheaval from release of aqueous vapour.

But even this, which I believe is the explanation of part of the diurnal waves as observed on continents, will not explain the variation observed at Mauritius, a small island situated in the heart of the South Indian Ocean, nearly 3000 miles from Australia and with the 1400-fathom line not more than 10 miles from the coast. No land-tide which may have been formed over the continent of Australia could be propagated so far westward under the sea, and the island itself is so small that the Sun's heat may be considered of equal strength on the east and west coasts at a given hour.

Now at Mauritius it has been found that the mean amount of evaporation is about two-tenths of an inch of water daily, representing a removal of 20 tons per acre, which, we will assume, creates in the bed of the ocean a slight rise; this will be propagated from east to west, and will cause the boom to behave as in our former hypothesis: but any such evaporative effect is practically *nil* in comparison with that of the ordinary lunar or solar tides; the former may perhaps cause a depression of the ocean-bed immediately under the place of high water and thus cause a lunar bi-diurnal variation, but this question has not yet been investigated. It is, however, evident that it would be very much greater than any solar effect, be it gravitational or evaporative.

There is another element to be considered in connection with the release of strain theory, viz.: the diurnal variation of atmospheric pressure, which at Mauritius amounts to 0·07 in. of mercury, or ·0023 of the whole pressure; this is equal to 2·9 lbs. per sq. yd., more than half the ordinary evaporative effect. As this change of pressure is of a bi-diurnal character, its effect (if any) on the diurnal variation of level would be shown as a bi-diurnal wave, and if we subject the Mauritius results to harmonic analysis we obtain

the following expression for the diurnal variation of level (reckoning from Mauritius midnight):—

$$dV=2''\cdot61\sin(\theta+295^\circ\ 47')+0''\cdot73\sin(2\theta+331^\circ\ 57')$$
$$+0''\cdot30\sin(3\theta+272^\circ\ 57'),$$

showing a comparatively large bi-diurnal term, which points to a possible connection between change of level and barometric pressure, the harmonic expression for which is:—

$$dP=0^{\text{in}}\cdot0108\sin(\theta+49^\circ\ 32')+0^{\text{in}}\cdot0285\sin(2\theta+163^\circ\ 2')$$
$$+0^{\text{in}}\cdot0020\sin(3\theta+26^\circ\ 4'),$$

with absolute maxima at $9^h\ 8^m$ A.M. and $10^h\ 0^m$ P.M., and absolute minima at $3^h\ 44^m$ A.M. and $3^h\ 14^m$ P.M., while in the level curve the bi-diurnal effect reaches its easterly maxima at $9^h\ 56^m$ A.M. and P.M., and its westerly maxima at $3^h\ 56^m$ A.M. and P.M.

It may here be mentioned that on the morning of January 7, 1899, with a falling barometer (during the passage of a cyclone), the boom moved gradually out of range towards the west, completing an arc of a little more than 8 seconds between midnight and 6 A.M.

This would seem to corroborate the above, though it may be only a coincidence. In the centre of a cyclone, where the barometer frequently falls two inches, the release of pressure is enormous, and it is a well-known fact that at sea the water in this area rises considerably above the level of the surrounding ocean; and the same action must take place on land—in a less degree, it is true, but with sufficient energy to materially affect even the crudest of horizontal pendulums.

I believe the theory of a solar "land-tide" has found but little favour among seismologists; but if, as the records of the Mauritius seismograph have shown, such enormous changes of level can occur locally from contraction and expansion of the Earth under the action of the Sun's heat, why should not a similar effect be produced by the same action on the Earth generally? Its magnitude would be extremely difficult to determine, though we may assume it would be very small. The elimination of local effects would also be no easy matter, and would necessitate several installations in close proximity and under varying conditions: for instance, Prof. Milne found different effects on different sides of a hill and in instruments placed above or below the level of the ground; he is also using an east and west boom as well as a north and south.

Until similar experiments have been made at Mauritius no discussion of the results can be considered as satisfactory or complete.

With regard to the gradual tilting of the boom pillar towards the west, as pointed out in a previous article, it should be mentioned that a corresponding tilting occurs in a north and south line, as shown by the progressive change in the sensibility of the instrument. As the boom points to the north, an increased sensibility indicates a tilting towards the south, and from the observed scale values we find that from the beginning of October to the end of December 1898 the pillar continued to tilt towards the south at a varying rate, then rapidly towards the north till January 16, and remained steady till the end of the month.

T. F. Claxton.

On Zinger's Method of Determining the Error of a Time-Piece.

When making the preparations for equipping the Russian parties for the Transit of Venus of 1874, it was found that the use of a portable transit-instrument for determining the errors of the time-pieces was excluded, from the fact that enough such instruments were not available. Lieut.-Col. Zinger, now Professor in the Nicholas Military Academy at St. Petersburg, then a junior astronomer at the Poulkova Observatory, was able to employ a suggestion in the 'Berliner astronomische Jahrbuch' made many years previously (1784) by an astronomer, Köhler, which had never been employed, and to make of it a thoroughly practical use. Köhler's suggestion was the employment of corresponding altitudes of different stars, as, for instance, δ Persei and α Cygni.

These two stars reach the same altitude, about 0^h of sidereal time—δ Persei on the east side of the meridian, and α Cygni on the west,—and many existing instruments can be readily arranged for the exact determination of the instant when the equal altitude takes place. Prof. Zinger's memoir was published in Russian, and for the convenience of astronomers a German translation, vouched for as exact by Otto Struve, was made by Heinrich Kelchner, a diplomatist acquainted with both languages, and published by W. Engelmann.

The instrument necessary for the observation was a very simplified zenith-telescope, and Zinger found that with little trouble the necessary arrangements could be made and the equipment provided. In his own practice he used two instruments, a Repsold vertical circle, and a smaller, less accurate Ertel theodolite, and in that way he showed that the method sufficiently provided for the practical necessity of equipping the various parties with instruments which were available and were adequate to the same accuracy of observation which the more familiar use of the portable transit-instrument afforded.

A later similar method was developed by Mr. F. Diaz Covarrubia, a distinguished Mexican astronomer; but the writer is not familiar enough with the Spanish language, in which it is published, to express a definite opinion as to its relative merits.

With respect to Zinger's method, his own experience with it has been favourable, and he would have no hesitation in recommending it to his pupils, and hopes by their help to present, at a future time, some definite results. T. H. Safford.

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*Prof. Darwin's Book on the Tides.**

The rough fact represented by the casual observation that the water by the sea-shore is at a high level twice in 24 hours, at times which vary from day to day, and the observation of the coincident fact that these times of high level happen at approximately the same time when the Moon's age is the same, led to the search for a reason connecting the two phenomena, and hence for a physical cause. This is typical of the beginning of most branches of science. The next step is to make accurate observations to compare with deductions from a propounded theory; but in the case of the Tides empirical laws were deduced before a satisfactory theory was formulated, and it cannot be denied that at the present moment the branch of the subject which deals with prediction of tides from observation is in advance of the branch which treats of their theoretical cause. The accuracy of tidal prediction may be gauged from the fact that the observed time of high water has sometimes differed by as much as an hour from the predicted time; but this difference is small compared with what it would be if the prediction were based wholly on either of the physical theories.

Prof. Darwin's book treats of both branches of the subject. It is not written for the advanced mathematician, but is the publication of a series of lectures delivered by him at the Lowell Institution, Boston, in 1897, and hence the physical causes of the tides are confined almost wholly to three chapters. Chapter V. gives a general explanation of the law of attraction and the force which generates tides, with ingenious diagrams. A later chapter sketches the Equilibrium Theory, much as it has been done in other places. The best that can be said for this theory is that it is in rough agreement with the facts. Spring-tides occur at full and change of Moon, and neap-tides at the quarters; but the actual times of high water can only be found by applying the correction for "establishment of the port"; and as this correction is sometimes as much as 6 hours, the theory is in such cases as much wrong as right in respect to the time of high water; but Prof. Darwin, when comparing it with the dynamical theory, says: "Notwithstanding, however, this condemnation of the equilibrium theory, it is of the utmost service in the discussion of the tides, because by far the most convenient and complete way of specifying the forces which act on the ocean at each instant, is to determine the figure which the ocean would assume if the forces had abundant time to act."

The depth of the ocean, and the rate of propagation of waves according to the depth of the water, are important factors in the other theory, the dynamical. This theory may be best explained from examination of a special case considered by Newton, in which he supposed the sea confined to a canal circling the Equator, and that the Sun and Moon move exactly in the Equator. To quote from Chapter IX. p. 148, as follows :—

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* John Murray, Albemarle Street, London. 1898.

> An earthquake or any other gigantic impulse may be supposed to generate a great wave in this equatorial canal. The rate of progress of such a wave is dependent on the depth of the canal only, according to the laws sketched in Chapter II., and the Earth's rotation and the Moon's attraction make no sensible difference in its speed of transmission. If, for example, the canal were 5 kilometres (3 miles) in depth, such a great wave would travel 796 kilometres (500 miles) per hour. If the canal were shallower the speed would be less than this ; if deeper, greater. Now there is one special depth which will be found to have a peculiar importance in the theory of the tide, namely, where the canal is 13¾ miles deep. In this case the wave travels 1042 miles an hour, so that it would complete the 25,000 miles round the Earth in exactly 24 hours. It is important to note that if the depth of the equatorial canal be less than 13¾ miles, a wave takes more than a day to complete the circuit of the Earth, and if the depth be greater the circuit is performed in less than a day.

In actual fact the cause which generates this wave, called a free wave, is either the solar tide-generating force, which, as shown in the equilibrium theory, tends to make a wave-crest at those points of the Earth's circumference where it is noon or midnight, or the lunar tide force, which has an analogous effect. At each moment of time the Sun is starting a new free wave, such as above described; and if the depth of the canal be such that the period of these free waves is a solar day, the theoretical effect would be that the tidal wave would accumulate and rise to a very great height. It is needless to say that this does not happen in nature, and the case is not considered further. This may be, perhaps, sufficient to indicate the method of the theory. The result of the superposition of waves of different periods is discussed, without elaborate mathematics, by Prof. Darwin, in an interesting chapter, from which the following extracts are made :—

> When the canal is less than 13¾ miles deep the Sun's disturbing force has, as before, a period of 12 hours; but the period of the free wave is more than 12 hours, because a free wave would take more than 12 hours to get half round the Earth. Thus the general principle shows that where the forces are trying to make high water there will be low water, and *vice versa*. Here, then, there will be low water under the Sun and its antipodes, and such a tide is said to be inverted because the oscillation is the exact inversion of what would naturally be expected. All the oceans on the Earth are very much shallower than 14 miles, and so, at least near the Equator, the tides ought to be inverted. The conclusion of the equilibrium theory will therefore be the exact opposite of the truth near the Equator.

And, again :

> If the whole planet were divided up into canals, each partitioned off from its neighbour, and if the canals were shallower than 14 miles, we should have inverted tides in the equatorial region and direct tides in the polar regions ; and in one of the canals in some middle latitudes very great tides, the nature of which cannot be specified exactly.

But though the mathematical theories represent the circumstances of Nature so imperfectly (and this can be said with no sense of be-littling mathematical science, for the subject is complicated to the extreme), it is possible to predict the incessant variations of the level of the ocean with much accuracy by discussion of observations. Whewell, in writing of the inductive method of research, said :—"The course which analogy would have recommended for the cultivation of our knowledge of tides would have been to ascertain, by an analysis of long series of observations, the effects of changes in the time of transit, parallax, and declination of the Moon, and thus to obtain the laws of phenomena, and then to proceed to investigate the laws of causation." The first part of this paragraph is a statement of the methods followed by tide-predicters. A century ago the method of tide-prediction was considered a secret, to be handed down as a valuable heritage from father to son, and the publication of new tables, accompanied by a statement of the method of calculation, was resented as an infringement of the rights of property. Mr. Lubbock (the elder) was the first mathematician of renown who realized that the computation of tide-tables was a science; and in 1831 he caused a nineteen years' series of tidal observations made at the London Docks to be analyzed, and therefrom produced tables, the early errors of which were joyfully pointed out by the secret holders, but which were eventually made more perfect than any previous predictions. Since then the science has been improved, by Prof. Darwin himself, by Lord Kelvin, and by Mr. Edward Roberts of the Nautical Almanac Office, who has done much to perfect the Harmonic Analyser, the instrument used in computing tide-tables. It may be explained that the tidal variations at any particular port happen according to some law, though it may be a complicated one, and have a definite period. In general these variations will be found to be caused by the superposition of several simple waves, sometimes as many as twenty, of different amplitude and period; and it is the function of harmonic analysis to evolve these elementary waves from the observed result of their combination, and hence give means of foretelling future circumstances.

In thus hinting at the main points of the book no mention has been made of several interesting matters contained therein. There is a very interesting chapter on *seiches* in Lake Geneva. A *seiche*, it is perhaps necessary to explain, is a species of wave caused by a rocking of the body of water of the lake as a whole. The height of a seiche wave is variable. M. Forel, who has made extensive study of the subject, has detected a seiche not more than a millimetre high; but there is on record an historical seiche with a double amplitude of as much as four feet. Prof. Darwin also gives accounts of attempts by himself and others to detect evidence of the distortion of the Earth's surface and variation of the plumb-line due to tidal causes. The additional weight of the water caused by high tide may be considered to cause a sort of dimple in the land, and so alters the slope of the land near the sea-shore, and at the same time the displacement of attractive matter alters the direction of the attracting forces. The latter portion of the book is devoted to the consideration of several branches of speculative astronomy to which the theory of the tides has some relationship. The variation of latitude with which Chandler's name is associated is discussed as being the result of a tidal force. There are chapters also on the generation of satellites and the evolution of the shape of planets as being figures of equilibrium of rotating bodies endowed with gravity, together with an excursus on Saturn's rings. Each of these is a subject in itself, and Prof. Darwin's lucid descriptions can scarcely be abbreviated. H. P. H.

Two Fine Perseids.

The Rev. S. J. Johnson, of Bridport, writes that on August 10, at $10^h\ 14\frac{1}{2}^m$, a very fine Perseid, brighter than any planet, appeared between the clouds and momentarily illuminated the heavens. "It started 3° south-east of Polaris, and shot 15° to the westward."

The same meteor was seen by the Rev. T. E. R. Phillips, of Yeovil, at $10^h\ 14^m$. He rated it as equal to Venus, and recorded the path as from 34° +74° to 225° +66°.

It was also observed by Mr. T. H. Astbury at Shifnal, Salop, at $10^h\ 14^m$, and judged by him as equal to Sirius. The duration of flight was about two seconds, and the nucleus left a streak which remained visible 12 seconds.

At Bristol I recorded a brilliant Perseid at $10^h\ 14^m$ on the same night. It was quite equal to Venus, though a part of its course was hidden by clouds. The end and denser portion of the streak was projected on a region of clear sky and endured 27 seconds. Its duration of flight was estimated as 1·1 second.

Comparing these observations, the radiant-point is indicated at about 41° +57°. The height of the meteor when first seen was 76 miles over a point 3 miles west of Cheltenham, and when it disappeared 40 miles over a point 4 miles north of Bridgwater. The length of its apparent path was 54 miles, and the earth-point 10 miles east of Plymouth. The velocity is uncertain, but it was probably about 32 miles per second.

Another large Perseid appeared at $10^h\ 54^m$. It was seen by the Rev. S. J. Johnson, who describes it as brighter than 1st mag., and moving in a path from 5° east of α Persei to a point 7° nearer the eastern horizon. At Yeovil the same object was noted by the Rev. T. E. R. Phillips, who estimated it as equal to Venus, and gave the path as from 37½° +48° to 35° +37½°. At Shifnal Mr. T. H. Astbury registered the object as 1st mag., and the flight from 357° +28° to 343° +4°. At Bristol the meteor was seen by the writer, and the observed course was from 23½° +38° to 17° +29°. The nucleus was as bright as Jupiter, and it left a pretty dense streak for several seconds at a place 3° S. of β Andromedæ.

The radiant-point was at about 45° +58°, and the height of the meteor at its commencement 97 miles above a place 3 miles N. of Mildenhall, Suffolk; at its disappearance the height was 51 miles above a point 3 miles S. of Chesham, Bucks. The length of its apparent path was 79 miles, and the earth-point near Ringwood, Hants.

Many other bright Perseids have been recently observed at more than one station. The very clear weather which fortunately prevailed on the nights from August 9 to 14 inclusive enabled the shower to be well observed this year. It was not a very conspicuous one, certainly not so rich as that observed on 1898 August 11, but the meteors were fairly numerous, and some of them of the most brilliant class. At Bristol 425 meteors were seen at the August epoch, and the largest of them all appeared on August 12 at $12^{h}\ 1\frac{1}{2}^{m}$, shooting from 4½° +43° to 347° +27½°. It left a broken streak, one section of which, near the end point of the meteor, remained visible to the eye for 3½ minutes.

Of the minor showers the one most actively pronounced has been from a radiant at 277° +70° in Draco. It furnished slow meteors, traversing short paths, and most of them were seen in positions far distant from their radiant. I saw a few meteors from the same radiant between July 4 and 14, but have never previously seen it so well defined at the time of the Perseids.

Bristol, 1899, August 21. W. F. Denning.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Constellations Equuleus, Hercules, and Ophiuchus.

Gentlemen,—

Interest having revived lately in the names of the ancient constellations, perhaps a few remarks on the three above mentioned may be acceptable to some of your readers.

Equuleus, of course, means a foal or little horse; but this small constellation is called by Ptolemy and the Greek astronomers *ἵππου προτομή*, "the horse's face," and on maps it is represented as such, almost as if Pegasus had dropped one head and, in a backward movement, acquired another. Tycho, I believe, was the first to use the expression "Equuleus," adding "Equi sectio," presumably translating *προτομή* by "sectio"; but, whilst etymologically a fore-section, as applied to an animal, the word means "face."

As regards the constellation Hercules, it is well known that it was anciently called Engonasin, signifying a figure on its knees, though, indeed, only one knee is represented as bent. Aratus disowns all knowledge of what the name of the figure thus depicted is; but Avienus tells us* that the greater age of Panyasis enabled him to discover this secret, and that the celestial figure in question was the famous hero of many labours, son of the wife of Amphitryon. Panyasis was a native of Halicarnassus, and a relative (probably uncle) of the historian Herodotus; he wrote an epic poem on Heracles or Hercules; but as only a few fragments of his writings have been preserved, we are dependent upon Avienus, in his Paraphrase of Aratus, for all we can know on this point. He informs us that Hercules, after he had plucked the golden apples in the garden of the Hesperides, overpowered the guardian serpent, and held it down under his left knee, Jupiter afterwards decreeing that he should be represented in that posture in the heavens.

Panyasis lived two centuries before the time of Aratus, so that we must suppose either that the latter was not aware of the remarkable discovery made by the former as to the personality of the Engonasin, or that he refused to accept it, and even to mention it, which seems scarcely likely.

Uncomfortable as the situation of Hercules in the sky (holding down in perpetuity a serpent or dragon with his knee) would appear to be according to mortal ideas, that of the figure whose head is almost in collision with his, though his body stretches in the reverse direction (towards the south), is even more so; for he not only holds a serpent in his hands, but plants his feet upon a scorpion. The word Ophiuchus, the serpent-bearer, has three separate forms in Latin—Anguitenens, Anguifer, and Serpentarius. Ideler informs us, on the authority of Golius, that the word used by the Arabs to represent it means, not Serpent-bearer, but Serpent-exorciser. The ancients seem generally to have understood by him Asklepios or Æsculapius, who is generally represented with a snake. Why Milton locates the constellation "in th' arctic sky" it is difficult to say, unless he confounds the serpent with the dragon, and Ophiuchus with Hercules. I have shown in another place ('Notes and Queries,' 7th ser. ii. 66) that when he compares Satan to a comet which

"\. . . . from his horrid lair
Shakes pestilence and war,"

it is not unlikely that he is alluding to the comet of 1664, which appeared about the time of the declaration of war with Holland, and was shortly afterwards followed by the first outbreak of the great plague.

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* 'Phænomena Aratea,' v. 172–6:—

Ille laboranti similis succedat imago
Protinus, expertem quam quondam dixit Aratus
Nominis, et cujus latuit quoque causa laboris
Panyasi sed nota tamen, cui longior ætas
Eruit excussis arcana exordia rebus.

Yours faithfully,

Blackheath, 1899, Aug. 1. W. T. LYNN.

The Thirtieth of February.

GENTLEMEN,—

A thirtieth day of February occurs in some early English Calendars, and perhaps a lingering habit of calling the last day of February by this number may account for the inscription in Cartmell Church.

Hampson's 'Medii ævi Kalendarium' contains reprints of several of these early Kalendars. Amongst them the MS. Galba has the following at the end of February: "Memento quod anno bissextili lunæ Februarii·xxx dies computas." Then we come to another Kalendar, termed the "Saxon Codex Vitellius E XVIII," much injured by the fire at the Cotton Library in 1731. With respect to the age, it is supposed to be a composition of the year 1031. In this work we have, at the end of February, the words: "Anno bissextili lune Februarii mensis xxx computandæ sunt."

There is also a small MS. in the collection, marked "Titus D XXVII," which, from internal evidence, belonged to a Saxon monastery. In this the same words as in the last, "Anno bissextili," are repeated.

Faithfully yours,

Melplash Vicarage, Bridport. S. J. JOHNSON.
1899, Aug. 5.

On the Tables in the Prayer-Book for finding Easter.

GENTLEMEN,—

The perusal of Mr. Cowell's interesting and elaborate paper on the Tables in the Prayer-Book for finding Easter suggests to me to ask the question: Why should Easter continue to be a movable festival at all?

A more eminently unscientific arrangement than the existing one it would be hard to devise. It is not as though an attempt were made to make Easter-Day synchronize in some way with the actual Lunar Phase. The so-called Paschal Moon has no existence in heaven above, in the earth beneath, or in the water under the earth; and is as purely an artificial creation as the hippogriff of the herald.

I should be really glad to know what objection can exist to making Easter-Day a fixed festival, falling upon one definite Sunday in every year, and so obviating all necessity for the elaborate calculations entailed on us by mere ecclesiastical tradition and prejudice.

Yours faithfully,

Forest Lodge, Maresfield, Uckfield. WILLIAM NOBLE.
1899, Aug. 8.

Multiple Publication of Results.

GENTLEMEN,—

I presume that my friend of the "Oxford Note-Book" is away from home, as I have been myself, so I address you on the subject of the letter of Diogenes which he has printed; I only regret that the remarks were not stronger, for it does not note that the proofs of papers placed on the table of the Royal Society at their meetings are intended for the use of Fellows, and to enable them to take an intelligent part in the discussions: they are not publications, and, if I am not mistaken, a note to that effect is placed on the table where they are made available. I think that the name of the person who has been guilty of the gross act which Diogenes brings to notice should be published, that all may know it and take precautions against his proceedings in future.

I am Gentlemen,
Yours faithfully,
J. F. TENNANT.

11 Clifton Gardens, Maida Hill, W.
1899, Aug. 21.

OBSERVATORIES.

PARIS.—The annual report of the Paris Observatory for 1898 was presented by M. Loewy to the Council on February 25, 1899.

Meridian Observations.—The change of most importance in the last year or two in the work of the Observatory has been in the direction of the more scientific organization of the meridian work. It has been found necessary to obtain greater stability for the instruments, and during 1898 piers of 12 or 13 metres depth have been substituted for the old piers of the *Cercle méridien du Jardin*, and as the instrument possesses two circles, a second set of six microscopes has been mounted, so that both circles are read at each observation. This instrument has been devoted to observations of latitude according to the method of M. Loewy. The latitude obtained from 387 determinations made in 1897 and 1898 is

48° 50′ 10″·40 in the direct position of the instrument.
48 50 10 ·15 in the inverse " " "

These positions are corrected for flexure by determinations made in December 1897, and a mean result of 10″·65 is obtained.

With the *Grand Cercle méridien* or the *Cercle du Gambey* the Sun, Moon, and major planets have been well observed during the year. With the former instrument a large number of observations of fundamental stars have been made, and with the latter instrument the revision of the stars of Lalande's Catalogue has been industriously pursued. In all 14,177 meridian observations were made during the year.

Large Equatorial Coudé.—During the year no less than 591 photographs of the Moon were taken. Most of these would not bear any enlargement, and only such plates have been preserved which verified the existence of the most delicate details shown in previous photographs. The Report contains a reproduction of a beautiful photograph of the Moon (age $20^d\ 5^h$) taken in September 1894.

Other work undertaken with this instrument has been the measures of diameter of small planets and satellites by M. Hamy, using an *interference* method.

Equatorials.—A large number of observations of the positions of nebulæ, comets, planet Eros and other minor planets have been made by MM. Bigourdan, Callandreau, and Fayet.

Photographic Chart.—Practically all the photographs have been taken, and the catalogue plates are being measured at the rate of 100 a year. The total number of plates measured to the end of 1898 is 538. The mean number of stars per plate measured in 1898 is 316, and varies from 74 to 1500. The reduction to right ascension and declination has been actively pursued, and nearly 23,000 determinations have been made of the stars on 62 plates. Reproductions on an enlarged scale of a number of the Chart plates have also been made.

The third part of the Photographic Atlas of the Moon, by MM. Loewy and Puiseux, has been published during the year. M. Bossert has continued the reduction of the observations of 'l'Histoire céleste,' using Von Asten's tables, and his determination of proper motions of Lalande's stars. The volumes of observations for 1889 and 1890 have been distributed, and an attempt is being made to bring the printing up to date.

NATAL.—It is not unusual in Reports from Colonial Observatories to meet with complaints based on insufficiency of means provided by the governing authorities, but it would be difficult to find a parallel to the state of things described in Mr. Nevill-Nevill's Report for the year 1898. He says

> When the appointment of Government Astronomer was made in 1882, it was stated that a residence would be provided for him at the Observatory, and pending its erection the hiring of a temporary residence was authorized, the rent to be paid from the Observatory vote. A short experience proved this arrangement to be highly inconvenient; the hired residence was given up and the allowance for rent suspended. Pending, therefore, the erection of the residence for the Astronomer, he had himself to take steps to build a bedroom so as to enable him to reside at the Observatory, though all meals had to be taken in the open air under the shelter of a verandah. Subsequently it was found necessary to extend the accommodation by the erection of a small dining-room and a tiny kitchen adjoining. For years, there being no room for the assistants, all computations and other work had to be done in the open air under cover of the verandah, the assistants dodging from side to side to obtain shelter from the wind and rain. This became so harassing that it had to be ended by the Astronomer himself providing a computing-room and small office by enclosing portions of the verandah and hardening the floor of sand by means of concrete and cement.

It appears that the Natal Government could not afford to build the Astronomer's house because they were making a railway to Johannesburg; but now that this is done a sum of money is to be provided for Observatory purposes, but not sufficient, Mr. Nevill-Nevill thinks, to build a suitable house, so that he will have to complete this at his own expense.

The Staff consists of the Director and his wife, who occupies the post of Senior Astronomical Assistant, a Junior and a Meteorological Assistant. These find their time fully occupied in making the transit observations necessary for the time service, in making the meteorological observations, and keeping the institution in an efficient state. No astronomical work was done during last year besides these time observations, except an unsuccessful search for Encke's Comet, some observations of Coddington's Comet, and a search for the planet Eros, which was seen but not observed. The observation of declination of stars by Talcott's method has been suspended, as the limited staff is not sufficient to effect this, and the Director's lunar investigation has been put away until such time as money is provided for its publication.

ROYAL OBSERVATORY, CAPE OF GOOD HOPE.—The equatorial mounting and the object-glasses of the McClean telescope reached Table Bay on 1898 April 11. The instrument is now set up (we derive our information from Dr. Gill's Report for 1898, dated 1899 January 18) and the dome completed, with the hydraulic motor for raising the floor and rotating the dome, so that the observer standing at the eyepiece can control its movement and the elevation of the floor with the utmost ease and delicacy. The clock is automatically wound, without any vibration of the instrument, by a hydraulic gear designed by Mr. McClean. The 18-inch visual object-glass appears to require no alteration, as its spherical and chromatic corrections are practically perfect, but the 24-inch photographic objective will have to be put in the hands of Sir Howard Grubb again, as the marginal images show *coma*, and the minimum focus is for rays of refrangibility about midway between $H\beta$ and $H\gamma$.

The foundations for the new transit-circle have been built, and the observatory itself, which is a construction of sheet steel made by Messrs. Cooke and Sons, of York, was soon expected, as was also the instrument from Messrs. Troughton and Simms, at the date of the Report.

The work of the old transit-circle has been chiefly devoted to the observations of standard stars required for the reduction of measures of the Astrographic Catalogue. During the year 1898, 10,355 transits were observed, and 9863 zenith-distances measured, with an adequate number of observations for adjustment of the instrument, which it is unnecessary to enumerate in detail, though it may be noted that 21 observations to determine flexure were made, and 175 observations of azimuth of a meridian mark. It is expected that these observations, which will form a catalogue of 9000 stars, will be finished in 1899.

The heliometer observations of the major exterior planets at opposition, mentioned in last year's Report, have been begun. The heliometer has also been employed in continuing the triangulation of 21 stars in the neighbourhood of the South Pole, and a series of observations have been made in the attempt to determine whether atmospheric chromatic dispersion has any systematic effect on heliometer measures. The answer appears to be in the negative.

With the equatorials 148 separate phenomena of occultations were observed during the year, 34 of them by more than one observer. The 7-inch equatorial is used by Mr. Innes for the examination of certain stars necessary for the revision of the Cape Photographic Durchmusterung, and in the course of the work has found many variable and double stars. The 6-inch equatorial, with a Zöllner photometer attached, has been used for comparing photographic and visual magnitudes in regions near the Pole and near the equator of the Milky Way.

As to the astrographic work, 199 Chart plates, with triple exposure of 30 minutes each, have been taken, and 200 "revision" Catalogue plates. Other plates taken for special purposes, and some incomplete chart plates, bring the whole number taken during the year to 469. In explanation of the term "revision" plates it is stated that it is proposed to repeat the whole series of Catalogue plates, in order to bring the epoch at which the plates were taken nearer to that when the comparison stars were observed. 27 Catalogue plates, containing 9066 stars, have been measured, and a second measuring instrument has been ordered from Messrs. Repsold.

The Cape 10-year Catalogue of Stars for 1890 was passed through the press in 1898, and will shortly be in the hands of astronomers. The current reductions are well advanced, as will be judged from the statement that the computation of meridian observations in 1898 is complete, and the ledgers formed to 1898 December 31. Arrears of reduction are being overtaken.

The Geodetic Survey of South Africa is proceeding.

Mr. S. S. Hough, Fellow of St. John's College, Cambridge, was appointed Chief Assistant on September 14 *vice* Mr. Finlay, who retired on August 28. Houses have been erected for the Assistant, Mr. Lunt, and the Senior Second-class Assistant, Mr. Pett. Plans have been approved for a new Physical Laboratory and Record Room.

PUBLICATIONS.

Remarkable Eclipses [*].—It is but three years since the first edition of this little work appeared, and it has now reached the

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[*] By W. T. Lynn, B.A., F.R.A.S. Fourth edition, revised. Stanford, 1899. Sixpence.

Fourth. The work has been brought up to date, and contains an outline of the results obtained at Nova Zembla in 1896, and in India in 1898. There is also a brief notice of important coming eclipses, commencing with the Spanish one of May 1900, and extending to that of June 29, 1927, which is total for a few seconds across the north of England. In this edition a paged index of all the eclipses referred to is introduced, which makes the book more useful.

PRIMITIVE CONSTELLATIONS OF THE GREEKS, &c.*—Tribute has already been paid in the pages of the 'Observatory' to Mr. Brown's erudition and range of research in connection with one of his numerous works, a translation in verse of the 'Phainomena or Heavenly Display of Aratos.' He stands forth among modern archæologists as the champion of Semitic or non-Aryan influence as the explanation of most if not all of the early so-called Hellenic legends; but, as is so often the case with writers who can lay claim to a set of ideas of distinct freshness or originality, he appears to treat this like the proverbial Hobby and ride it, not only to death, but to Tartarus and the Elysian fields as well.

The established fact of precession *proves* that the origin of the mapping of the constellations must have been some fifteen or sixteen centuries before the era of Greek civilization and in a different latitude; and since, at that epoch, the only centre of civilization that we know whose latitude is suitable was the Euphrates Valley, it requires no credulity to attribute that origin to Chaldæan or Babylonian sources. But Mr. Brown, in the present work says "I am not concerned in this work with matters astronomical except incidentally," and so, with the ostensible object of arriving at the same result from a different standpoint, has accumulated an immense quantity of evidence, some apparently relevant, and probably much that appeals to the philologist, but is not within the ken of the general reader, to whom the book professes to be addressed.

In the view of the general reader it is almost inconceivable that practically every constellation and every star of note should be taken to represent either the Sun or the Moon. Such wearisome monotony may have been the case with those old-time sages who mapped out the heavens, but it seems in the last degree improbable. The great Solar Myth is responsible for much perverted ingenuity.

To quote an instance. Mr. Brown remarks, *quasi ex cathedrâ*, "The comparison of the Sun to a ram or bull is a line of thought which naturally and spontaneously arises in the mind of Archaic man"; and again, "The connection in idea between the Moon and the bull, ox, or cow is so obvious as to be inevitable."

Now without professing to fathom the probable workings of the mind of archaic man, it does seem likely that the crescent Moon appeared to resemble the horns of a bull, but why must the Sun also be a bull? And why, if so, should Mr. Brown find it necessary to restrict the Sun arbitrarily to the ram, except that it must be made to fit the alternating hypothesis (the Bull following the Ram as the night the day), and as an additional illustration of what he calls the Law of Reduplication? But to return to the general scope of the work. The author, after first proving that the star-list of Ptolemy is in reality that of Hipparchus, gives a translation of it with profuse philological and speculative notes. He also traverses Greek literature, and separately Homer, in search of references to constellations, and gives a very long list of coin-types of Phœnicia, Carthage, Cyprus, Lydia, Lycia, Etruria, and Greece, ranging from B.C. 800 onwards (none of antiquity at all approaching the known date above referred to), to prove that then, more than 1000 years after their invention, the constellations were commonly known to civilized man.

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* 'Researches into the Origin of the Primitive Constellations of the Greeks, Phœnicians, and Babylonians.' By Robert Brown, Jun. Vol. i. Williams and Norgate. 1899.

Then, after a chapter on *un*numismatic art in the same direction, Volume I. concludes with Babylonian Astronomy after Alexander, identifying the ten antediluvian kings of Berosus with ten bright stars in or near the ecliptic, and reconciling some apparently hopelessly large numbers by calling them intensifications of the smaller numbers—a truly magnificent apology.

Some parts of the work are eminently readable, in spite of the *correct* spelling of names. The author spells ancient names after a fashion of his own, and then argues that this is *not* pedantic. The Greeks were quite justified in Hellenicising names, and surely an English writer should aim at something less hopeless than an exact spelling with a totally different alphabet. Mr. Brown's parallel with Louise de Querouaille is beside the mark, as French is not a dead language and we do know how the French pronounce it. He argues that it is equally pedantic to play Macbeth in the costume of the period represented. Why not call it pedantic to ride an 1899 pneumatic because bone-shakers were in use some thirty years ago? W. W. B.

NOTES.

COMET NOTES.—Swift's Comet (1899 I.) was observed at Geneva till June 30, when it was just bright enough for accurate measurement. Signor Cerulli succeeded in obtaining an observation on July 31, which is the latest published. The comet was then very faint, with a nucleus of the 14th magnitude (Ast. Nach. 3586). It is now probably beyond the reach of any telescope.

No further observations of Holmes' Comet are to hand, but we continue the ephemeris by H. J. Zwiers, from Ast. Nach. 3582; it is for Greenwich noon:—

	R.A. h	m	s	N. Dec. °	′		R.A. h	m	s	N. Dec. °	′
Sept. 1....	3	3	38	40	16	Sept. 21....	3	9	36	44	45
5....	3	5	53	41	13	25....	3	9	3	45	32
9....	3	7	39	42	9	29....	3	7	53	46	16
13....	3	8	52	43	4	Oct. 3....	3	6	4	46	56
17....	3	9	32	43	56						

'Bulletin Astronomique' for August contains a careful discussion, by M. L. Schulhof, of the orbit of Tempel's Second Periodic Comet (1873 II., 1878 III., 1894 III., 1899 *c*). He previously announced that this comet showed an acceleration in its mean motion, but he has found a numerical error in his computations which explains the apparent acceleration.

The following are his predicted elements for this year, after applying planetary perturbations :—

$$\begin{array}{l}
\text{Epoch 1899 June 18·0 Paris M.T.}\\
\begin{array}{lrrr}
M \ldots\ldots\ldots & 352^\circ & 26' & 30''
\end{array}\\
\left.\begin{array}{lrrr}
\omega \ldots\ldots\ldots & 185 & 35 & 59\\
\Omega \ldots\ldots\ldots & 120 & 58 & 46\\
i \ldots\ldots\ldots & 12 & 38 & 52
\end{array}\right\} 1900{\cdot}0.\\
\begin{array}{lrrr}
\phi \ldots\ldots\ldots & 32 & 49 & 42\\
\mu \ldots\ldots\ldots & 671''{\cdot}886 & &
\end{array}
\end{array}$$

The observations last May show that the above value of M requires a correction of about −17″. He expresses the hope that a long series of observations will be obtained at the present apparition, which will enable a new value of the mass of Jupiter to be deduced.

'Comptes Rendus' for August 14 contains observations of this comet made by M. G. Fayet at Paris on July 31, Aug. 9, 10. On the first day the comet was pretty bright, 4′ in diameter, with a stellar nucleus of the 10th magnitude. It was more difficult to see on the other days, but this was chiefly owing to its increasing south declination.

'Bulletin Astronomique' for August contains a very useful catalogue of cometary orbits by M. G. Fayet. The elements are reduced to 1900, and the orbits are arranged in the order of increasing inclination from 0° to 180°, elliptical and parabolic orbits being given in separate lists. This arrangement will greatly facilitate the enquiry whether the elements of a new comet show any resemblance to those of any previous comet. The catalogue is brought up to date, even Swift's comet of the present year being included.

A. C. D. C.

OBITUARY. — G. C. PULSFORD, F.R.A.S. In most of the British aud Colonial observatories there will be found a member of the staff who received his earliest astronomical training at the Greenwich Hospital Schools—to these we announce with deep regret the death of the Headmaster. The sad event occurred almost suddenly at Salcombe during the vacation. Mr. Pulsford was 56 years of age.

THE ZODIACAL COINS OF THE EMPEROR JAHĀNGIR.—The issue of 'Knowledge' for July contains an article by Mr. E. Walter Maunder on the above subject, illustrated by a fullpage plate with photographs of 28 of the coins in question. The coinage of the Emperor Jahāngir is noteworthy for two remarkable issues: the one in which the uxorious monarch caused the name of his celebrated queen Nūr Jahān to be engraved on the money together with his own, the only instance of the name of a queen-consort appearing thus on an Indian coin: and the other in which a representation of the sign of the zodiac, corresponding to the month of issue, is stamped on the obverse, a circumstance hardly less remarkable, as Muhammadans generally regard themselves as forbidden to represent natural objects by sculpture or engraving. These two most unusual issues in the same reign no doubt gave rise to an interesting legend related by Tavernier the traveller, and here given in full by Mr. Maunder, in which the two issues are confused together and described as having been struck in a single day by the order of Nūr Jahān. No doubt, however, her influence had much to do with the astronomical rupees as well as with the others, and, indeed, her name occasionally appears upon them.

The coins themselves present several points of interest. All the twelve signs of the zodiac are extant in gold mohurs struck at Agrah, and all are represented, some by two or more forms, in the plate in 'Knowledge.' The rupees struck at Ahmadabad likewise originally showed all the signs, but several are not now to be found in any known collection. The designs are in many cases clearly European in character; Aries and Taurus in particular. Others follow the more distinctive Indian forms; thus Taurus is sometimes a humped Indian bull, full length, and not the demi-bull in the act of charging which we have on our celestial globes. The Twins, again, are a male and a female figure; not two young men as with us. Sagittarius, the finest design of the set, is shooting his arrow over his back, in the Parthian mode; whilst an ordinary Indian *lotah* on some of the coins represents Aquarius.

The engraving, which, by the courtesy of the British Astronomical Association, we are permitted to reproduce from the recently published report on "the Indian Eclipse, 1898," represents all the forms, with the exception of Pisces, of the silver rupees struck at Ahmadabad which are known to be still extant. The eighth coin shows the inscription on the reverse.

To those interested in the study of the surface-features of Mars we can heartily recommend the joint paper by MM. Flammarion and Antoniadi in No. 3581 of the 'Astronomische

Nachrichten.' The title is "Observations de l'Aspect physique de Mars durant l'Opposition de 1898–9," and with it are two plates, one showing the general chart of Mars and the other six typical drawings.

From an Oxford Note-Book.

If the reflecting telescope had powers of observation and reflection in a sense not usually intended, it would be amused at the various things said about it, especially by Directors or Acting Directors of the Lick Observatory. It will be remembered that Mr. Crossley some years ago presented to the Lick Observatory the 3-foot reflector which he had purchased from Dr. Common, and with which the latter had made the first successful photographs of nebulæ (about 1882–1884). The Assistant told off to work with it failed to get any good results, and complained of the instrument as antiquated, and of his Director for asking him to work with it. The latter replied, if I remember rightly, that he had publicly accepted the instrument and it must be worked. Then Prof. Schaeberle, who on Prof. Holden's resignation acted as Director, proved geometrically that there was a radical defect in all reflecting telescopes. Finally, Prof. Keeler, on succeeding to the Directorship, has used the Crossley reflector for an important and useful purpose—to obtain photographs of nebulæ with the visual part of the spectrum for comparison with drawings, and calls it "an instrument which possesses in the highest degree all the qualities required" for this work. And yet all these remarks are not inconsistent. Prof. Schaeberle's geometrical objection to the instrument is sound enough in its way, though it does not affect the usefulness of the reflector so seriously as might be at first imagined: and he has, indeed, himself pointed out that the objection is less serious for some lines of work than others. And there are other defects of the instrument which are well known to those who have used it much. A man of experience has compared it to the feminine sex for variation in amiability, and consequent necessity of "humouring." Prof. Hale and Sir H. Grubb have both said things on the subject lately, but I have unfortunately at the moment mislaid the references to their papers.

It is rather unfortunate that most people take their holiday at the same time, so that American friends who call to see us in August find that we are away from home—possibly even calling upon them. Sometimes it has been my lot to stay at home in August, and the experience was in many ways most pleasant—it is an excellent time for work (and play too); and one sees visitors whom one would otherwise have missed. When the pressure of continual engagements is relieved, there is a special pleasure in unexpected or irregular meetings with friends. There was an out-of-session meeting of the R.A.S. Council the other day—a necessary formality for the signing of some document; I was prevented from attending myself, but I hear that though there was little business to transact there was a large amount of conversation, as might have been forecast.

PERHAPS, though, it is a little forgetful of the Southern Hemisphere to say that most people take their holiday at the same time. The Southern August falls, I suppose, in February; and we may thus expect southern visitors about the time of the R.A S Annual General Meeting, the date of which was probably fixed with a view to this future possibility. As yet these welcome visitors have been so few that I can scarcely say whether this is a general rule; but as they multiply we may expect a regular influx at the Annual Meeting.

It may be some time before the Southern Hemisphere establishes a similar Annual Meeting in *their* February—our August—for us to attend in exchange; but there are hopeful signs of such a possibility on the Australian continent; and we may, in time, migrate with the cricketers. Again, when the Transvaal question is settled we shall hope for a crop of observatories in South Africa to lessen the inequality of distribution in the two hemispheres; and presently the South African observers will find the need of meeting together at times. The meetings on these vast continents will probably not be so frequent as those in England, but annual as in Europe and the States; for in the States the method of Annual Conference seems to be practically established; there have been two thoroughly successful conferences, and a third is just about to be held, at which a scheme of permanent organization is to be discussed.

ANOTHER sign of holiday times is the dearth of material for making notes out of. (Why does this recall the remark that it "would not be possible to make railways in Canada because there was nothing to make tunnels out of"?) I find myself scanning the daily press with more than usual care for items of astronomical interest. The name of Mars caught my eye last night in a copy of the *Yorkshire Evening Post*, but I am not sure that the information conveyed about the planet will be considered important; some, indeed, may regard it with suspicion or even with downright scepticism. Hence I include for comparison the preceding and following statements, which, with that about Mars, are extracted from a long series of facts offered for digestion to the readers of this paper. It will be noticed that only the third statement is made in decided terms: the first is only a "belief of many people," and if they should be misguided the popularity of Southport will have to be increased by other methods than that of advertisement. The second statement, which particularly interests us, is given on the authority of an American paper; and we know from experience of weather forecasts that information coming from America, though often successfully correct, goes astray at times. But there seems to be no doubt about the whisky—unless we doubt the word of the *Yorkshire Evening Post.* Here, then, are the three statements *verbatim et literatim* :—

> Many people believe that Southport only needs greater advertisement to enjoy greater popularity.
>
> An American paper states that a young lady's hair has, by the influence of Mars, been turned green.
>
> In future the Kitchen Committee of the House of Commons will mature their own whisky. The vat will hold 700 gallons.

UNDER these circumstances (that is, in the dearth of material mentioned in the last paragraph) I shall venture to take a considerable risk and print two letters received several weeks ago, which I have hitherto held back because I could not sufficiently see the point of them, as I will presently explain. First, however, I give the letters just as received:—

University Club (Dublin),
July 5, 1899.

To MR. OXFORD NOTE-BOOK,

DEAR SIR,—

You, as an Oxford man, know that *orbis* does not mean a sphere, but a round disk or a circle, and that when the Romans used *orbis terrarum* they meant that the Earth was a round flat disk, just as Mr. Breach (I think that is his name, I have not now my *Observatory* lying before me) would say. It might be hypercriticism to object altogether to the use of this expression in these days, though founded on ignorance and now so very unsuitable. But, *pace tanti viri,* I do think that Dr. Sandys should have avoided it when introducing Cornu to get his honorary degree at Cambridge the other day (see *Nature,* June 8, p. 128). He says that Cornu measured the mass of the *orbis terrarum.* He might as well speak of the mass of Cambridgeshire or the specific gravity of mathematics.

Your obedient Servant,
SCREWTATER.

Dublin, July 13, 1899.

DEAR SIR,—

Did you ever happen to notice what I have copied on the next leaf? I accidentally observed it only last night.

Your obedient Servant,
SCREWTATER.

(*Jane Eyre,* Chapter V.: first two sentences.)

Five o'clock had hardly struck on the morning of the 19th of January, when Bessie brought a candle into my closet and found me already up and nearly dressed. I had risen half an hour before her entrance, and had washed my face, and put on my clothes by the light of a half-moon just setting, whose rays streamed through the narrow window near my crib. (About a page lower down): The moon was set and it was very dark . . . . . . It wanted but a few minutes of six.

(*Jane Eyre,* Chapter XII.: one third down, just before the meeting with Mr. Rochester.)

I lingered till the sun went down amongst the trees, and sank crimson and

clear behind them. I then turned eastward. On the hill-top above me sat the rising moon: pale yet as a cloud, but brightening momently.

Now with regard to the first letter the criticism seems to the point, though I wish Mr. Screwtater had told us the correct word to use. When the cricket professional, asked why a particular sort of ball was called a "yorker," replied that "He did not see what else you could call it," he was, perhaps, lacking in appreciation of the resources of a living language; but Dr. Sandys might use this reply when challenged as to his name for the Earth with greater safety, for the words of a dead language, like Latin, are as strictly limited as the coins of the Romans. To find a correct Latin phrase for the mass of the Earth would seem to me as hopeless a quest as that for a coin with date B.C. 65 on it; but I really ought not to venture any opinion on this matter, for I am no scholar. With regard to the second letter, however, I am really puzzled; for Mr. Screwtater evidently detects a mistake in Chapter V. which I cannot see, and so have not dared to print his letter earlier. I cannot ask him what it is, since he writes anonymously; and to pretend to see the mistake might end in disaster if some one else equally dense asked me for the answer and I had still not found it. So I now offer the puzzle just as it was sent me—"find the mistake,"—with the frank acknowledgment that I have not yet found it myself.

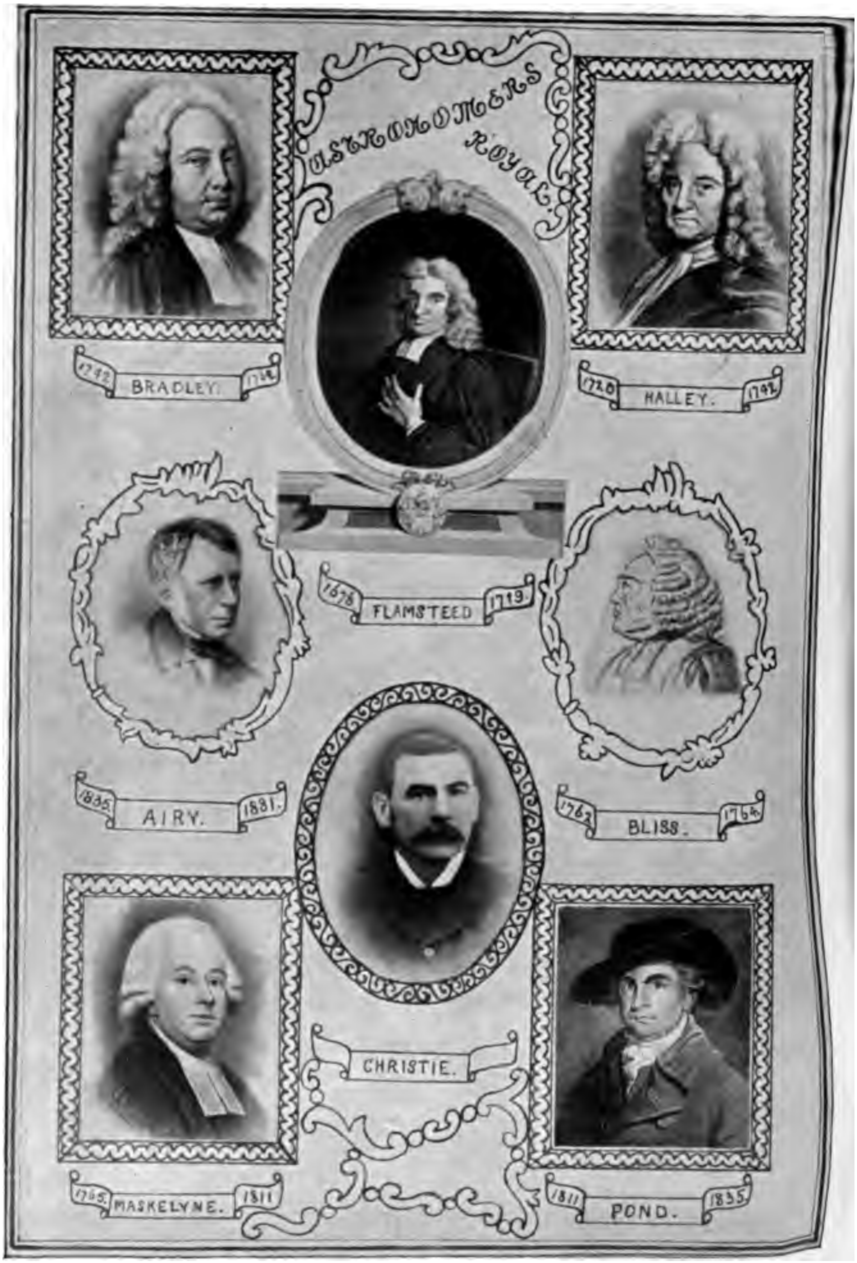
Nor am I much clearer about the extract from Chapter XII. Mr. Screwtater says about it:—

> I noticed this only this morning. I believe this is a quite common mistake. I have come across more striking instances of it in conversation; and have sometimes failed to disabuse the observer's mind of it.

Is the critic objecting to the picturesque inversion of the Moon brightening instead of the sky darkening? If this is all, I am a little happier about the mistake in Chapter V., which may be no greater; it may be that the half moon was only a quarter moon or something of that kind.

TALKING of correspondence, I wonder if I have been churlish in taking no notice of the following request from the United States? It seems to me rather a cool request, with no sufficiently alluring inducements attached; but I may be wrong:—

> DEAR SIR,—
>
> In the preparation of a work on Astronomy we want very much the portraits of Dr. James Bradley, Joan Joseph Leverrier, Joseph Louis Lagrange, Hipparchus, and Maria Clerke, none of which can we find in this country, and it is of the utmost importance that we have them at once, and if you can furnish us with any or all, we shall gladly reimburse you for the expense you may entail. We are ready now to go to press, and it is a heavy expense to delay our work, but we shall do so until we hear from you. If you can do nothing better than to have photographs taken of paintings or statuary please do so, and have bills sent to us C.O.D. If you can favour us in this regard your efforts will be thoroughly appreciated. Thanking you in advance, and hoping we shall hear from you by return mail if possible, ——



THE ASTRONOMERS ROYAL.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXII. OCTOBER, 1899. No. 284.

The Astronomers Royal.

THERE may seem no special reason for the publication at this particular time of the frontispiece to this number, for the reputation of the eight men whose pictures are there given is not ephemeral; but the completion of the new building at Greenwich, on the entablature of which their names are inscribed * in the most honoured places, suggested the collection of the lineal presentiments of the astronomers who form the dynasty of Greenwich Observatory. Any attempt at detailed histories to accompany the portraits would be merely to repeat what has been most efficiently done in other places, but it may be well to add some salient points of the life and work of the subjects of the pictures.

JOHN FLAMSTEED came to the Observatory a young man, 30 years of age, with a Cambridge degree, and in holy orders. The circumstance that he was appointed to the living of Burstow soon after he was made Astronomer Royal, and that he inherited a private income, somewhat mitigates the fact that his salary was only £100 a year. The general opinion about astronomy in his day is indicated by a sentence in a letter he wrote to Wren, inviting Wren and Newton to dinner. He says:—"The gentlemen of our Society (the Royal), however ingenious and well wishers to all sorts of ingenious study, know little of mine." Possibly this feeling of want of sympathy induced in some way the querulousness he showed in his relations with Halley and Newton, although it has been said that Flamsteed's greed of money was the cause of his troubles with these men. Flamsteed observed positions of the brighter stars, the Sun, Moon, and major planets for 42 years. His results are contained in two volumes of the 'Historia Cœlestis,' of which there is a so-called corrected edition by Halley. Flamsteed's observations of the Moon were used by Newton as material for making his lunar theory. His star-positions were collected early in this century by Mr. Francis Baily to form the British Catalogue of (3310) Stars. Flamsteed spent part of his time in teaching pupils. It was part of his official duty to instruct two boys from Christ's Hospital in mathematics and navigation, and besides these he had others—dukes, lords, and captains of vessels, one biographer says. He had as assistants Abraham Sharp, the renowned instrument-maker, and Joseph Crosthwait. The names of three men who acted as computers—Hudson, Leigh, and Ryley—are recorded. On the ceiling of the Painted Hall at Greenwich Flamsteed is depicted with Thomas Weston, who was probably a pupil. Our portrait of him is copied from the engraving which forms the frontispiece to his 'Historia Cœlestis.'

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* The name of the present Astronomer Royal is not on the building.

EDMUND HALLEY was a man of higher attainments than Flamsteed, but he was not so well fitted for his office, for in the first place he was 63 years of age when he was made Astronomer Royal, and secondly he was not a good observer. His work at Greenwich may be summed up by saying that he erected the first transit-instrument in the Observatory, and also put up the iron quadrant, so that he may be considered as having created the type of two instruments which are at present combined in the Greenwich Transit-Circle. His observations are of little value, but his reputation was made before he came to Greenwich. Halley was an energetic, many-sided man—courtly, well known at court, and evidently able to use influence in high places. He was second only, and a good second, to Newton in gravitational astronomy. He was the first of the many English astronomers who have been to the southern hemisphere to observe, for as a young man he sojourned at St. Helena for this purpose. He went to Dantzic to discuss with Helvelius the merits, as a measuring-instrument, of the then newly-invented telescope, as compared with the old method of sights: above all, he was far in advance of his time in appreciation of terrestrial magnetism as a science, and obtained from King William III. the use of a vessel for a voyage to the South Seas in 1694 to make magnetic observations. This voyage, however, was not successful. Halley was Savilian Professor of Geometry at Oxford from 1704 to 1742, so that he held this office concurrently with that of Astronomer Royal. Through the good offices of Caroline, Queen of George II., who visited the Observatory in 1727, he drew half-pay as a Commander in the Navy, having at some time held the King's commission. Our portrait is from an engraving in the Royal Observatory.

JAMES BRADLEY was 50 years of age when he came to Greenwich in February 1742, and he married shortly after. He had already made the first of the two great discoveries, that of the aberration of light, by which his name will be ever remembered. Twenty years before he had been made Savilian Professor of Astronomy at Oxford, and he held this office till his death. He was a clergyman, but, as the Oxford statutes forbid the Professor holding a clerical appointment, he resigned two church livings to take up the professorship. He was offered the vicarage of Greenwich when he came to the Royal Observatory, which he did not accept; but the salary of £100, which still pertained to the office of Astronomer Royal, was increased by a pension of £250 granted Bradley by the Crown, and which was continued to Bliss and Maskelyne. By the end of his first five years at Greenwich he had made the series of observations which established his second great discovery, the nutation of the Earth's axis, and in the whole 20 years of his tenure of office he made an excellent series of observations of the stars observed by Flamsteed, used by Bessel to form his 'Fundamenta Astronomiæ,' and re-reduced by Dr. Auwers to form a catalogue, whose value for comparison with those of modern days can scarcely be over-estimated. Among Bradley's Assistants there were two of some renown: Charles Mason, who went to America to lay down the boundary-line between the provinces of Maryland and Pennsylvania, and measured a degree of the meridian in that neighbourhood; he observed the Transits of Venus of 1761 and 1769, but specially he edited Mayer's lunar tables for use in the compilation of the Nautical Almanac. The other was Charles Green, who was Assistant also to Bliss and Maskelyne, and went with Captain Cook on his first voyage to the Southern Seas, which was undertaken to observe the same transit. Green died on the voyage home *. Bradley's death is entered in the Transit-book and in the Quadrant-book, after the observation of the Sun on 1762 July 16 (probably made by Charles Green):—"Dr. Bradley exchanged this life for a better at Chalford, in Gloucestershire, July 13, 1762. Aged 70." The books were then closed, and no more observations were entered therein. This is mentioned to show how personal to the Astronomer Royal the Greenwich observations were considered. How jealously Flamsteed guarded his observations is well known. On Bradley's death his executor, Rev. Samuel Peach, took possession of his observations, and there were actions-at-law, first by the Royal Society and then by the Board of Longitude, to recover these. Mr. Peach was successful, however, and then presented the observations to the University of Oxford. Bliss's observations also had to be purchased from his widow by the Board of Longitude, and these were published with Bradley's and those made by Green (see below) by the University. Our portrait of Bradley is from an engraving, the original of which is an oil-painting in the Octagon Room at Greenwich.

The career of NATHANIEL BLISS (1762–64) at Greenwich was too short for him to have left any mark on the institution. He

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* Bradley's other Assistant, and first in order of time, was his nephew, John Bradley. There is some doubt as to whether Gael Morris, who helped Bradley with his lunar work, should be considered his Assistant. He was probably not on the Greenwich staff. The annual salary of an Assistant was then £26.

was more than 60 years of age when he received his appointment. Like his two predecessors he was an Oxford man, and had succeeded Halley in 1742 as Savilian Professor of Geometry. Our portrait of Bliss is copied from a small print in the Hope Collection * of the Bodleian Library, which is believed to be the only portrait extant, and to which a curious story is attached. About the year 1750 Lord Parker, who, better known as Earl of Macclesfield, has a reputation as an astronomer and patron of the sciences, was giving a dinner at Shirburn Castle, near Oxford, to some scientific and artistic celebrities of the day, among them being Bliss and J. Skippe, an expert etcher and engraver. Over the walnuts and wine the host challenged Skippe to draw a portrait of Bliss, whereupon Skippe took up a pewter mug and graved this portrait on the bottom. From the mug the prints, of which there are three now in the Bodleian, were made, and in these there is an inscription round the head not reproduced in our picture, which runs, "This sure is Bliss, if Bliss on earth there be."

Bliss was succeeded by **Maskelyne**, whose first observation was made on 1765 May 7, after an interregnum of seven months, during which time Charles Green had charge of the Observatory. It has been said of Maskelyne, as of many others, that *he was the founder of modern astronomy*, the statement being justified by the fact that he was the first to make a *fundamental* star catalogue; he limited his observations to a select number of stars (besides, of course, the Sun and Moon and planets), which he observed on every possible occasion, and produced a catalogue, the precursor of the Greenwich Clock-Star List, containing only 36 stars, but the places of which were known with the greatest accuracy. He was the first to observe transits to tenths of seconds. Maskelyne's great work was the establishment of the 'Nautical Almanac,' which was prepared under his direction until his death. It is said that Maskelyne was of so modest and retiring a disposition, that when the king visited the Observatory he left his assistant † to entertain the royal party; but this seems improbable. He died on February 9, 1811. The original of our picture is believed to be in the possession of the Royal Society.

Maskelyne was the first to add to the dwelling-house of the Astronomer Royal, which until 1790 remained entirely as designed by Wren. But about that year the three eastern rooms on the south side of the Astronomer Royal's house were built. To those who know the Observatory as it now is, the following statement of the gradual growth of the Observatory proper will be intelligible. Flamsteed's sextant occupied the site of the instrument now known as the "old Altazimuth." Halley's Transit-room was probably a small room at the west end of the terrace which overlooks the river (not the room now covered by a dome); his quadrant was probably on the eastern side of the west wall of what is now known as the old computing-room, in the place where Bradley's brass quadrant now hangs. Bradley added the old computing-room, with the room over as a living-room for Assistants, and the room that Sir George Airy and the present Astronomer Royal occupied as their official room as his Transit-room. The present Transit-circle room was built in 1808 or 1809 by Maskelyne, forming the second of the gable-ends seen from the front court. Pond added the Sheepshanks Dome and the small rooms now used as Assistants' Rooms, in which his Chief Assistant lived entirely, and the Library. The range of buildings was completed by Airy, who added the Record Room and the South-east Tower. Three living-rooms were added at the West of the house for Airy.

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* We are much indebted to Mr. F. Bellamy, of the Oxford University Observatory, for this reproduction and for the following story. Also to the Curator of the Hope Collection for the necessary permission.

† The names of Maskelyne's Assistants, most of whom only stayed for a short period, and one only at a time, are as follows:—Joseph Dymond, William Bayley, Reuben Burrow, John Hillins, George Gilpin, Joseph Linly, Thomas Horrox, Robert Gooch, George Flowman, William Malachy Hitchins, John Brinkley, John Burnstead, Joshua Moore, William Garrard, William Smith, John Crossley, Benedict Chapman, Joshua Garnett, David Kinebrook, Thomas Evans, Robert Wallace, Francis Nisbet, Thomas Ferminger, Thomas Taylor.

Maskelyne was succeeded by JOHN POND *. Although his name is little known, indeed probably not at all outside the ranks of professional astronomers, he was one of the ablest of the men who have filled the chief office at Greenwich. He was a perfect master of meridian instruments. In the year 1806 he had shown that the Greenwich quadrant had changed its form since Bradley used it, which was subsequently verified by Troughton; he invented the method of observing by reflection from the surface of mercury; and the excellence of his observing has lately been proved by Dr. Chandler, who has established his theory of the variations of latitude mainly from Pond's observations. He did a great deal for the Royal Observatory: he equipped it with good instruments, he established a system of reducing observations, and he increased the staff from one to six assistants †. It was said, at his death, that he did more for practical astronomy than any other man since Bradley, and it seems hard that his latter years should have been embittered by a factious clique who wanted his post for their own disposal. He resigned office towards the end of 1835, and died September 1836, aged about 69.

Of the two remaining occupants of the office it becomes us to say little; as has been said, Airy completed the range of buildings

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* Our portrait of Pond is believed to be rare. It is copied from a print in the possession of a Greenwich townsman, whose family have lived in the neighbourhood for some generations. We are indebted for this, and also for much help in the preparation of the picture, to Mr. Bowyer of the Royal Observatory.

† The *one* Assistant was Thomas Taylor. The *six* were Taylor, Henry (Belville), Richardson, Ellis (senior), Rogerson, F. Simms.

at the north end of the Observatory domain: he also added smaller and less permanent buildings in other parts of the grounds, as they were required by the addition of branches of work to the Observatory scheme. It was left for Mr. W. H. M. Christie to replace these by a building more worthy the nation and more suitable for the conduct of the daily work. It would be idle and unfair to compare the work of the present day with that of the time of the earlier astronomers. The main difference, apart from the increase in the number of branches dealt with, is that the observations are now reduced and published shortly after they are made, whereas it had to be one of Airy's first tasks to reduce the lunar and planetary observations of Bradley, Bliss, and Maskelyne. One of the annual Greenwich volumes of to-day is bigger than the two volumes which contain Flamsteed's work for forty-two years.

H. P. H.

E. W. Brown's Theory of the Motion of the Moon.

By this time Professor Brown's researches in the lunar theory require no notice from the point of view of those interested in the mathematical difficulties of a numerical lunar theory. The class is a small one, and every member of it is probably pretty well acquainted in detail with the work of Dr. Hill and Prof. Brown. We here attempt, however, to sketch an outline of this work, prefacing it with a popular account of the history of the subject, starting from very early times.

From the time of Copernicus the Sun has been recognized as the centre of the planetary system, and in some way responsible for its motion. Before Newton the prevalent opinion was that the Sun was responsible directly for the velocity of the planets in their orbits. In other words, given the Sun and the planet, it was inconceivable that the planet should not have its actual velocity. The mental attitude may best be conceived by the comparison of a vortex-motion in liquid. Stir a basin of water until a cylindrical hollow appears in its centre of a certain shape and a certain velocity of rotation. Then if a match be floating in the water between the vortex and the rim of the basin, its velocity is entirely determined by the circumstances of the moment. In the case of a falling stone, on the contrary, the velocity is determined by the time that the stone has already been falling. It is easy to conceive the same stone occupying the same position and moving in a different way. The match in the basin could not move in a different way. Newton demonstrated that the Sun is only responsible for the change of motion of the planets, and identified the cause that keeps the planets in their orbits with the cause that is responsible for the path pursued by a falling stone. Of course the philosophers who thought that the planets could only have their actual velocities, could conceive that no such condition was imposed on the stone. They did not see any analogy between the two problems. It was the nature of planets to move in circles, they thought, and it was the nature of fire to mount upwards, and it was the nature of a stone to seek the spot where the centre of the Earth was. There was no idea of the force of gravitation residing in matter. Dante, for instance, evidently supposes that all matter seeks to approach as nearly as possible to a certain spot, which the centre of the Earth has come to occupy in pursuance of this law. Dante would have considered that, if the whole Earth were pushed aside, it would come back again to the old position. The Earth was attracted, or rather had an innate desire to approach a certain spot; it was not itself the seat of attraction. Hence it is that Dante does not make the attraction fade away into nothing, as the poets approach Lucifer at the centre of the Earth; but, on the contrary, it is as strong as ever, but suddenly changes its direction as the centre is passed.

These ideas passed away on the discovery by Newton of the law of gravitation. Newton showed that the consequence of his law is that the planet or satellite would describe an ellipse round its primary except in so far as it is prevented by the attraction of third bodies. He also traced to the Sun the irregularities from elliptic motions in the Moon's orbit about the Earth.

Newton completely solved the problem of two bodies, or the motion of one body under the attraction of a second. The problem of three bodies, or the motion of a body under the attraction of two others, has never yet been solved, and probably cannot be. The recent researches of Prof. Darwin *, dealing only with a fringe of the whole subject, wonld almost warrant the belief that there is no path, however fantastic, that could not be pursued by a body under the attraction of two other bodies under proper initial conditions. Attempts at classification have been made by pure mathematicians, and the lines on which to proceed pointed out, but it must be acknowledged that the results are meagre in the extreme. Meanwhile it has been found possible to trace, with a high degree of universal accuracy, the consequences of Newton's law of gravitation in the actual cases that occur in the solar system. The labour is immense, but it has been performed in several different ways. Of all numerical calculations in celestial mechanics, the problem of the motion of the Moon is the most tedious, and the lines on which it is being attacked by Prof. Brown by far the best.

Tables have been constructed at various times by which the place of the Moon at any time can be found. These tables have been formed after long numerical calculations based on the special conditions known to obtain in the case of the Moon. In view of the extreme complexity the problem of three bodies is capable of assuming, it must be regarded as good fortune that tables are

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* See 'Periodic Orbits,' vol. xxi. p. 121.

possible. It is true that, with Newton's law of gravitation and with sufficient labour, the Moon's place could in any case have been predicted in advance by a sort of dead reckoning. To illustrate this, consider a piece of driftwood in the sea. If we knew a great deal more than we do know, it might become possible to predict where that piece of wood will go to in a week, and then starting again with the new position, the prediction could conceivably be extended to a second week, and so on; but tables could never be made, for two reasons. One is that prediction would soon cease to be accurate. An error of an inch at the end of a week would become two inches at the end of the next week, and would soon make all the difference whether, for example, the log went up the English Channel or up the Irish Channel. The second reason is that in general such a log would not find itself back near some definite point at regular intervals. To calculate a place for any time we must then calculate for all intermediate times. Supposing, however, we could describe some course that we could prove that the log never got very far away from, and supposing that we could always predict how far away from this fictitious course the log would be at any time, then tables would be possible. Now it happens that the Moon is so close to the Earth that the disturbing action of the Sun is never very large in comparison with the attraction of the Earth, and hence we conclude that the Moon will for a time not get very far away from the ellipse that it would describe in the absence of the Sun. In some respects, however, the action of the Sun is not wholly oscillatory in its effects, and the result is that no fixed ellipse will continue to approximately represent the Moon's path. The difficulty was generally got over by considering an ellipse rotating in a convenient manner. Such an intermediary orbit, as it is called, suffered from one great inconvenience. Without the Sun the Moon could describe any fixed ellipse. It cannot, under any conditions, admitting a physical interpretation, describe a rotating ellipse. Hence the idea of the ellipse merely got over the difficulties of making tables, but it did not contribute much towards the solution of the problem of the Moon's motion.

Dr. Hill's great contribution to the lunar theory lay in this. He proposed to abandon the rotating ellipse as the intermediary orbit and substitute another curve, which is called the variation curve, to play the same part. The Moon never gets far from this latter curve, so that it shares all the advantages of the rotating ellipse. It is equally simple in its geometrical properties, and it has, ove and above, this immense convenience. It is a curve that the Moon could actually traverse if the actual conditions were slightly modified. It is true that the Moon does not actually describe the curve. It oscillates about it in four distinct ways. One oscillation is due to the fact that the Sun, though a long way off, is nevertheless at a measurable distance; a second is because the Sun is nearer in winter than in summer; these causes give rise to what may be called forced oscillations. Besides this there are two free oscillations; for these no cause can be assigned; they happen to exist and that is all. The Moon, for instance, might have moved in the plane of the ecliptic, but it does not do so. It also might have moved in a curve much more nearly circular than it does.

The lunar theory is reduced then to a calculation of oscillations about a state of steady motion, and only differs in being more laborious from those problems with which every student of advanced dynamics is familiar, such as, for example, the rising and falling of a top, or the wobbling of a hoop or a bicycle when not going quite smoothly. The nature of the conclusions is at once anticipated, and the embarrassments disappear. Clairaut gave his name to an artifice for expressing symbolically that the nodes of the Moon's orbit regrede. The artifice seems simple enough to those to whom it has been explained, but it evidently was a great discovery once. Considering the Moon's motion in latitude as an oscillation, no one is surprised that the nodes regrede. It merely means that the Moon does not take the same time to oscillate from side to side of the ecliptic that it takes to go round the Earth. No one expects the period of a bicycle's wobble, for instance, to be exactly the period in which the wheels or the pedals go round.

It is not easy to describe, in popular language, the stage in the computations that Professor Brown has reached. Roughly speaking, he has perhaps done the smaller half. The whole will take five years at least. By former methods it has taken twenty to get a less degree of accuracy. The leading idea, as has been explained, was due to Dr. Hill, but Professor Brown's papers are full of ingenious methods for simplifying the work; but perhaps what strikes the reader most is the orderly arrangement so that no term in long calculations should be forgotten, so that every stage of the work should be tested for accuracy, and so that every set of numbers should be ready to hand as it is wanted. No muddle could probably be so bad as a muddle in the middle of a lunar theory, and it is no small achievement merely to have avoided one.

P. H. Cowell.

The Observatories of India.

By the courtesy of the Secretary of State for India, we are in possession of the Report of the Observatories Committee of the Royal Society, who have been considering, for the Indian Government, the Reports made by the Astronomer Royal and Sir Norman Lockyer, mentioned in our August number.

1. The Committee consider that the work of the Indian Observatories should be directed to the following subjects (for some of which provision is already made):—

(*a*) Astronomical observations (positions of Sun, Moon, planets, and fundamental stars). (*b*) Solar physics, as laid down in the reports of the Astronomer Royal and Sir J. N. Lockyer. (*c*) Magnetic observations. (*d*) Meteorological observations.

2. With regard to the astronomical observations, the Committee are informed that the meridian circle now mounted at Madras would (with some alterations) be available for this purpose, and the Astronomer Royal and Sir J. N. Lockyer are agreed that a suitable site for the Astronomical Observatory can be found at Kodaikanal.

3. The Committee suggest that a new spectroscope, as recommended by the Astronomer Royal and Sir J. N. Lockyer, be purchased without delay for observations on solar physics, together with an instrument for obtaining photographs of the surface of the Sun in monochromatic light. It is desirable that the daily photographs of the Sun be continued at Dehra Dun, at any rate for five years.

4. In view of the suggested magnetic survey, referred to in the next paragraph, the Committee approve the proposal to establish a permanent magnetic observatory in the north of India, and they understand that special facilities exist for this purpose at Dehra Dun.

5. The Committe approve, and attach great importance to, the suggestion that a magnetic survey of India should be undertaken, and enclose a minute by Professor Rücker on the conditions which should, if possible, be fulfilled. The magnetic survey should be under the general direction of a superintendent having special knowledge of terrestrial magnetism.

The Committee also recommend :—

6. That a small board of visitors, composed of a few scientific officials selected by the Indian Government, make an annual inspection of the Indian Observatories, and report to the Indian Government on their condition and administration.

7. That an annual report by the Government Astronomer of India and by the Superintendent of Magnetic Surveys be submitted to the Observatories Committee of the Royal Society.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Bradley's Observations at Kew.

GENTLEMEN,—

Now that Kew Palace is open to the public, it is probable that many more persons than heretofore will see the spot where Bradley made (in conjunction with Samuel Molyneux, to whom the house and instrument belonged) the first observations of γ Draconis, which, continued subsequently at Wanstead (where his uncle, James Pound, had formerly been rector), led to his great discoveries, first of aberration and afterwards of nutation. Molyneux became possessed of the house in 1721, in right of his wife, whom he had married in 1717, and who belonged to the Capel family. He set up the zenith sector of 24 feet radius, made by Graham, in 1725, and both he and Bradley, who had succeeded Keill as Savilian Professor of Astronomy at Oxford in 1721, observed with it from time to time, chiefly with the object of ascertaining whether they could trace any changes of apparent place due to stellar parallax. Delambre, wise (as so often happens) after the event, thinks these observations should from the first have been made more persistently. He attributes (as Rigaud points out) to Samuel Molyneux the authorship of 'Dioptrica Nova,' which was really the work of his father, William Molyneux, who died in 1698, and thinks that his observations received their impulse and direction (which there is no reason to suppose) from those of Picard, the latter having been the first to prove, on the occasion of his visit to Uraniborg (the scene of Tycho's observations) in 1671, that the changes of stellar place noted by Hooke were not due to parallax. Molyneux and Bradley began their observations at Kew in 1725, and terminated them in 1727, when the former was appointed a Lord of the Admiralty, an event succeeded in the following year by his death at the early age of 38. Although the house where the observations were made was pulled down in 1803, there is no doubt that the exact spot (on the south side of a stack of chimneys within the house when it stood) is marked by a sun-dial on a pedestal with suitable inscription, which was erected by order of William IV. in 1832; and, as I began by remarking, all those who now visit the existing palace (which came into the possession of Queen Charlotte in 1781) cannot help noticing the dial, as it is almost directly in front of the entrance to the palace, not more than fifty yards from the enclosure. I took an opportunity of seeing it on the 11th inst., and overheard some ladies discussing the inscription. "Aberration! what is that?" asked one. "Oh!" replied an elder lady, "it is the shadow—the shadow, you know!" waving her hand in a way which would explain all things. "And the nutation?" said another, "that means the Earth's axis does not go quite straight, doesn't it?" "Exactly so," was the reply, with another wave of the hand.

Yours faithfully,

Blackheath, 1899, Aug. 12. W. T. Lynn.

Early Observations of the Colours of Jupiter's Belts.

Gentlemen,—

In connection with the study of periodical changes in the colours of Jupiter's belts, I am very desirous of obtaining particulars of any early observations of the colours of the belts.

From the present time back to 1867 the record may be considered as more or less complete and satisfactory, but prior to this date there appear to be very few results published in a readily accessible form. Nevertheless there must be many observations extant, both published and unpublished, and I should be greatly obliged if any of your readers who may happen to know of, or come across, early records of the colours would kindly communicate or publish the same.

In a paper published in the 'Monthly Notices,' vol. lix. p. 376, will be found a table of the degree of redness of the two equatorial belts, which table is fairly complete for the period 1867 to 1899, though very deficient between 1867 and 1846. I am indebted to Mr. W. F. Denning for the undermentioned records of the colours in 1860:—

1860, April 20 & 25...	"S. equat. belt dull yellowish red."—J. Baxendell.
1860, March	"Belts on the S. half of planet are of a variety of yellowish red, while those on the lower or N. half are bluish, particularly the oblique belt."—J. W. Long.
1860, March 26	"In the region above the equator there was a double belt. The colour had a most decided reddish tinge—a pale brick-red."—Astronomer Royal, Greenwich.
1860	"The principal belt [S. equat.] red."—Secchi.

The above observations seem to show clearly that in the early part of 1860 the N. equatorial belt was at or near a minimum of redness, and the S. equatorial belt at or near a maximum of redness. The computed time of minimum and maximum, according to the formula in the paper above referred to, is 1860·63, so that the agreement could scarcely be more satisfactory. This makes altogether nine epochs of maximum and minimum redness which have been more or less well observed.

Other computed epochs of maximum and minimum for which there are no observations at present are 1855·57 (maximum redness of N. equat. belt and minimum of S. equat. belt) and 1848·55 (minimum redness of N. equat. belt and maximum of S. equat. belt). Are there not any observations of the colours of the belts in or about these years?

Yours faithfully,
A. Stanley Williams.

20 Hove Park Villas, Hove.
1899, Sept. 8.

The Gegenschein.

Gentlemen,—

With regard to the articles on the "Counterglow" of the zodiacal light in your last two numbers, may I point out that one part of Mr. Anderson's theory is clearly incorrect, and appears to be unnecessary for the remainder, namely, that which refers to the shadow of the Earth? No trace of the shadow has ever been seen in the counterglow, and therefore no effect of contrast can be produced by it. The absence of the shadow is simply accounted for by assuming so great an extension of the meteoric matter that the distance reached by the Earth's shadow is relatively short and unimportant. It is possible, however, that the Earth's shadow may have some effect in reducing the increase of light opposite the Sun. Comparing the counterglow with the reflection from blades of grass, no doubt the shining nature of the blades may increase the glow round the shadow of one's head upon them; but I would point out that a glow is visible round the shadow, not only upon grass, but upon other objects that are not at all shining. It is quite striking on ordinary broken soil, and in this case I suppose it is caused only by increased reflection of light through that part being more directly opposite the Sun. If, then, irregular particles of soil are capable of showing a striking counterglow, as they are, surely it may easily be believed that particles of meteoric matter may be capable of the same.

As Mr. J. Evershed points out, it must be difficult either to prove or disprove that the counterglow is related to the zodiacal band; but I cannot confirm his remark, on the authority of Professor Barnard, that the zodiacal band is not always present. On the contrary, my belief is that it always is so. My tabulation given in the 'Monthly Notices,' vol. xli. p. 334, indicates this, although the table is only a summary, in which the dates of the observations are not given, but only the Sun's longitude. The visibility of the band depends so much on the state of the atmosphere, as well as of the observer's eyes, that its absence or the variation of its intensity would be difficult to prove. As observers become more experienced in its observation, they see it more easily; on the other hand, after a certain period of life it is usual for the sight to deteriorate, so that it should become more difficult to perceive; it is doubtful whether the power of vision is ever quite stationary. For many years I invariably saw the zodiacal band if the night was at all suitable; only from the beginning of May until the latter part of August it cannot be seen here on account of its unfavourable position. It is plainly visible here in March and September.

West Hendon House, Sunderland,
1899, August 15.

Yours faithfully,
T. W. Backhouse.

The alleged Eclipse when Cæsar crossed the Rubicon.

Gentlemen,—

Although I showed in the 'Observatory,' more than eleven years ago (vol. xi. p. 154, No. 134, for March 1888), that the statement made by several writers that an eclipse of the Sun took place whilst Cæsar was effecting with his army that passage of the Rubicon which has given occasion to a proverb, and made that little river famous, was founded on error, and that no eclipse occurred within more than half a year of that time, yet it may be of interest to point out that the error was probably originally an inadvertence of 'L'Art de vérifier les Dates.' Speaking of the large eclipse of March 7, B.C. 51, the authors state that this was in the year of Rome 703, "tems auquel Jules-César passa le Rubicon, suivant Dion, liv. xli." Now Dion Cassius does not mention the passage of the Rubicon at all, and it is clear from him (xli. 4), as well as from other writers, that Cæsar's march from Gaul took place in the year of Rome 7045. Some time afterwards (xli. 14), speaking of the time when Pompey was at Dyrrachium, he enumerates several prodigies which he says took place in that year: one of these was an eclipse of the Sun, meaning in all probability the annular one of August 9, B.C. 49. Cæsar had probably crossed the Rubicon about the beginning of that year. As to the eclipse of March 7, B.C. 51, Petavius (Father Petau), who is also referred to *in loco* by 'L'Art de vérifier,' rightly says that it took place the year before the civil war began. After quoting Dion's mention of an eclipse he writes:—"Hoc verò anno antecedente belli civilis initium accidisse significat" ('De Doctrina temporum,' tom. ii. p. 259). He evidently considers that Dion meant the eclipse of March 7, B.C. 51 (of which he gives particulars in tome i. p. 819), but, as stated above, it is more likely that Dion intended the eclipse of August 9, B.C. 49, the year preceding the battle of Pharsalia. It would seem, then, that 'L'Art de vérifier' is responsible for the error (so often copied since) that an eclipse took place at the time of the passage of the Rubicon, the date of that event being about a year and nine months after the eclipse to which they refer it.

Yours faithfully,

Blackheath, 1899, Aug. 18. W. T. LYNN.

An enormous Meteorite.

GENTLEMEN,—

I beg to enclose the following, translated from a paper published in Porto Alegre, Rio Grande do Sul, Brazil, which may be of interest:—

"On the 12th February, at about 7 A.M., a noise was heard like the report of a cannon, followed by a slight trembling of the earth, and a distant sound like the rolling of heavy bodies down the precipices of the mountains. Those who, at the moment, looked towards the north, saw an immense igneous globe descending rapidly from the clouds. . . . . . It was then discovered that an enormous aerolite had fallen beyond the pass of Santa Barbara, on the Rio das Antas, Upper Taquary. It measured over 17 metres at the base, 26 metres in height, and as much in thickness, composed of a substance of the nature of iron, and weighing thousands of kilograms. . . . . .

"Fortunately it fell on a deserted spot on the slope of the right bank of the River Antas *, where it will remain until eternity"!

Yours very truly,

Rio de Janeiro,
1899, July 26. G. W. NICOLLS.

* Near Porto Alegre.

OBSERVATORIES.

MELBOURNE.—The date of visitation at Melbourne, which used to be in the early part of July, has been altered, and the function now takes place in March, in order that any financial proposals by the Visitors can be submitted to the Government before the preparation of the estimates. Mr. Baracchi's Report, recently received, covers, therefore, the period of only eight months, July 1898 to February 1899. As the work of the Astrographic Chart appears to have occupied much attention, we give the subject first place in this note. It may be remembered that Melbourne is responsible for the zone exactly antipodal to that being done at Greenwich, that is, for the region from the South Pole to declination $-65°$, while the Sydney Observatory has charge of the zone immediately below this, namely from declination $-64°$ to $-52°$, and that these two observatories have combined their resources so that the reduction of all the plates is to be done at Melbourne. At this latter observatory all the catalogue plates, 1149 in number, and 387 of the chart plates have been taken, and the measurenent of the catalogue plates is being begun. Six young ladies have been engaged to measure (there seems to be no lack of available female labour in the colony—the Director appears to have had numerous applicants), and 3500 stars have been completely measured on ten plates, which, including time spent on trials, learning, and incidental items inseparable from the beginning of new work, has occupied four months. There are three measuring-instruments at Melbourne, and at the beginning the method practised at Greenwich and Oxford was adopted—that is, the measures were made by means of a graduated scale in the eyepiece, and it was found that one micrometer could measure at the rate of a plate a day, which is about the same rate as at Greenwich: but Mr. Baracchi says that this method gives a probable error of a measured co-ordinate exceeding $0''\cdot5$; this has therefore been abandoned, and the measurements are now being made with a filar micrometer. Progress is found to be intolerably slow, but the probable error is found to be not more than $0''\cdot1$; so on the strength of Dr. Gill's opinion that satisfactory measures can be made at the rate of 40 stars per hour, the colonial Directors have, with the approval of the Chief Secretary, ordered from Messrs. Repsold a new machine of the pattern described in Memoirs R. A. S. vol. liii. The first remark that it occurs to us to make about this is, Why should it be made in Germany? and the second, Why should it be made at all? There is abundant evidence from experience that measurements can be made with the scale with the accuracy necessary for the Astrographic Catalogue. Why delay the work longer in striving for a problematical accuracy?

With the transit-circle a list of zodiacal stars, prepared by Dr. Gill for use in his heliometer observations, are being observed.

1571 observations of Right Ascension and 1017 of North Polar Distance were made in the period covered by the Report.

A sum of £100 has been put at the disposal of Mr. Baracchi for the reduction and publication of the observations of terrestrial magnetism made in previous years. This work is of considerable magnitude, as the total number of curves to be dealt with amounts to some 30,000. With the present staff about 4000 can be reduced in one year. The meteorology of the colony continues to be looked after with great energy, Mr. Baracchi is enlisting the services of country postmasters to observe earthquake phenomena, as these officials have means of noting accurate time; also he has been authorized to place in order the service of coast barometers which were put up for the use of the seafaring community, and, as though he had not enough on his hands, he is asked to undertake the testing of air-meters which are used in mines for measuring ventilation. We are glad to see that the finances of the Colony permit of an increased grant to the Observatory.

WINDSOR, NEW SOUTH WALES.—Mr. Tebbutt's Report for the year 1898 opens with this paragraph: "The year 1898 was remarkable for the large number of clear nights during the autumn, winter, and spring months. A large amount of work was therefore done.

Of this work, the first item to be noted is the cometary observation. The finding of Encke's comet on 1898 June 11 by Mr. Tebbutt has already been mentioned in these pages. Its position was observed on June 15 and June 25, but on June 26 and subsequent evenings, contrary to expectation, it was too faint for observation. Besides this, a very long series of observations of Comet Coddington-Pauly has been made. Its position was determined on 80 nights in 1898, and the series was continued into the current year; altogether it has been observed on 103 nights between June 15 and March 3. Other equatorial work consisted of observations of 36 disappearances of stars at the Moon's dark limb and measures with the filar micrometer of the relative positions of planets and neighbouring stars, 746 comparisons in all. Local time was determined by means of the Cooke 3-inch transit instrument on 168 nights. The instrumental errors used and the clock-errors found are given in the Report in some detail. It may be noticed that on the nights, of which there are a few, when the instrument was used in both positions, the azimuth error "clamp west" was generally greater than that "clamp east" by about 1″. It would be interesting to have the difference of clock-error found in the two positions similarly given. The Report also gives the *résumé* of the meteorological observations of the year, making altogether a total of work for the year of which the proprietor, Mr. Tebbutt, who practically does it all, may well be proud.

THESE brief reports of the work done at various Continental Observatories during the year 1898 have been abstracted from the 'Vierteljahrsschrift der Astronomischen Gesellschaft,' vol. 34.

ARCETRI. *A. Abetti.*—The observations of comets are:—

Comet 1898 ..	I.	IV.	V.	VI.	VII.	VIII.	IX.	X.
Observations..	42	33	8	18	14	4	15	25

Numerous observations of small planets, including 44 of planet Eros (DQ).

BAMBERG. *Ernst Hartwig.*—Diameters of Sun and Mercury with heliometer, and position of the crater Mösting A. A few observations of comets and variable stars. But with the refractor a large number of observations of variable stars were secured (571 obs. on 71 nights). 200 persons visited the observatory.

BERLIN. *W. Foerster.*—Besides the observations for Time Service, Herr Battermann made 1629 observations of R.A., and 1505 of Declination. The greater part of the Director's time was taken up in preparing for press his meridian observations made 1892–97, and in collecting material for determining the P.M. of 229 stars.

Prof. Knorre made micrometric measures of Stars in Duner's catalogue which showed no relative motion. He used a double-image micrometer.

BERLIN (Astronomisches Reicheninstitut). *J. Bauschinger.*—The principal work was the preparation and publication of the 'Jahrbuch' for 1901. The new constants adopted at Paris are used, as well as the Newcomb-Hill planetary tables, except for Mars. The 'Jahrbuch' for 1902 is in manuscript, and that for 1903 is well forward. After this comes the work on small planets.

BONN. *F. Küstner.*—The Repsold meridian-circle has been employed in continuing the observations for a catalogue with epoch 1900. 4508 complete places were obtained, besides 926 observations of 'Jahrbuch' stars and 89 of Pole stars. Besides these, 280 complete places of 18 stars in Hyades were secured for normal places to be used in the triangulation of the Hyades with the heliometer.

BRESLAU. *J. Franz.*—Considerable work on Lick photographs of Moon to determine Moon's figure and position of 150 craters. Observations of the Leonids gave the radiant-point

$$\alpha = 149^\circ{\cdot}63, \quad \delta = +23^\circ{\cdot}04.$$

During the lunar eclipses of July 3 and December 27 numerous contacts of shadow with craters were secured.

DÜSSELDORF. *R. Luther.*—Observations of small planets with ring-micrometer on 7-foot refractor, bringing the total number since 1848 up to 2151 observations of 213 small planets.

GENÈVE. *R. Gautier.*—Time-service. A few observations with the Plantamour equatorial. Lunar eclipses of January 7, July 3, and December 27 successfully observed. During the year 474 watches were deposited for trial, as compared with 578 in 1897. Meteorology.

GÖTTINGEN. *W. Schur.*—With the heliometer Prof. Schur made measures of 70 Ophiuchi and a few other double stars, diameter of Jupiter on three nights, and parallax-measures of 61 Cygni and Polaris. Prof. Ambronn continued the measures of Sun's diameter, making the total 572 single measures since 1890 May. The small heliometer was employed 16 evenings for measures of Struve double stars. A few comet observations were also made. The meridian-circle was used for time-service and observations of the planets Jupiter, Uranus, Saturn, and Mars. A discussion of the Sun observations since 1890 by Schur and Ambronn gives the diameter

Schur 1920″·11, Ambronn 1919″·76.

The Berlin 'Jahrbuch' adopts Auwers' value 1919″·63.

HAMBURG. *R. Schorr.*—The meridian work is only for the time-service. A number of small planets were observed, including Eros, on 12 nights. The comet observations are:—

Comet 1898....	I.	IV.	VI.	VII.	IX.	X.
Observations ..	18	3	1	2	2	12

The chronometer work for the German Navy is carried out here. Meteorology.

PUBLICATIONS.

FATHER SESTINI'S SUN-SPOT DRAWINGS.—Eight-and-forty years ago Father Sestini made at Georgetown Observatory a series of drawings of the Sun's surface, which was then in a state of unusual activity. The series extended from 1850 September 19 to 1850 November 6, and were first published by the kindness of Lieut. M. F. Maury, then Director of the U.S. Naval Observatory, in vol. iii. Appendix A of the Publication for 1847, printed in 1853. Many of the engraved drawings have remained since then stored away in the archives of the Georgetown Observatory, and this year the present Director, Father Hagen, S J., conceived the happy idea of issuing them to observatories which may not possess the early volume in which the engravings originally appeared. The student of the Sun's surface to-day experiences much the same interest in examining drawings such as these of half a century ago that a bibliophile has when he comes across a black-letter edition of the 14th or 15th centuries. In neither case can he hope to learn anything startlingly new; in both cases the interest lies in the human element, in showing forth the manner and style and idiosyncrasies of the translator or artist.

In Father Sestini's drawings the Sun's diameter is given on a scale of about five inches. Every variety of form of spot and stream seems to have been exhibited on the Sun's surface during the seven weeks that it was passed under review. There are regular spots and spots of every degree of irregularity, large spots, small spots, nucleated spots, and shaded spots; groups of spots and streams; long streams, straight streams, curved streams, parallel streams; streams where the principal spot leads, and streams where it follows. One specially complicated group was drawn on September 27, when it had recently passed the central meridian. A large, irregular, much-nucleated spot occupies the south-preceding position, and from its north-preceding side a large broken mass of spots has become detached. Following the group is a smaller nucleated spot, but the most curious is the fine reticule of lines that joins the large leading spot to the final one. So remarkably fine and canal-like are these lines that Father Sestini would certainly be open to the charge of infringing Mr. Lowell's patent if he had not had the good fortune to have been born some fifty years previously.

The most striking feature of the whole series, however, is the reduplication of nuclei and spots. The components of the duplex spot or nucleus are in all cases linear, parallel, generally equal, and, indeed, strongly resemble small portions of the reduplicated canals on Mars, with which we are so familiar, as represented on Schiaparelli's maps. Thus on September 19 there are six marked cases of reduplication; on September 20, six again; on the succeeding days sometimes a larger number, sometimes fewer. In most cases the line of division makes a considerable angle with the solar equator, the angle with the circle of declination being practically constant. This seems unusual, and we are at a loss whether to ascribe it to chance peculiarity of the period of observation or to some mannerism either in Father Sestini's drawings or in the method of their reproduction. One group offers a marked exception—the large group in the southern hemisphere which ran from September 20 to October 1, and in which the reduplication is parallel to the equator. The fact that this group stands out seems to indicate the objective reality of the phenomenon in the other cases, and the point is certainly worth considering in future investigations of the sun-spot forms. The doubling of spots and nuclei has, indeed, been noted in more recent maxima, but not with such frequency or in any constant direction as in Father Sestini's drawings. A. S. D. M.

NOTES.

Comet Notes.—No further observations of Holmes' Comet are yet to hand; the following is an approximate ephemeris for Greenwich noon:—

	R.A.			N. Dec.	
	h	m	s	°	′
Oct. 3	3	6	4	46	56
7	3	3	37	47	32
11	3	0	31	48	3
14	2	56	48	48	29
Oct. 19	2	52	28	48	48
23	2	47	32	49	0
27	2	42	2	49	5

The brightness reaches a maximum in the middle of October, when it will be 1·8 times that at re-discovery; it was then of the 16th magnitude, so that it will be very faint even when brightest.

The August number of the 'Publications of the Astronomical Society of the Pacific' contains some beautiful photographs of Swift's Comet (*a* 1899), taken at the Lick Observatory by Messrs. Coddington and Palmer. The tail was longest (9°) on May 10. On May 6 the tail consisted of one main branch 7° long, with side streamers. On May 7 there were numerous side streamers, and the main tail had a twisted aspect. On May 9 there was a sharp bend in the tail 3° from the head. On May 18 the tail was very slender and decidedly curved. On May 19 the tail had a curiously unsymmetrical aspect, as though one side of it had been destroyed. On June 5 there was a short stubby tail with a streamer on each side.

About June 9 two rudimentary tails appeared, inclined 15° to the main tail.

On all days the axis of the main tail pointed very nearly away from the Sun, the greatest deviation being 3°.

Mr. Perrine gives further details concerning the separation of the nucleus into two portions, and their subsequent recession from one another. He also gives some measures made to examine whether the refraction produced by the comet appreciably shifted the position of stars over which it passed. The answer is in the negative.

'Bulletin de la Société Astronomique de France' for September contains some drawings of the same comet made last May by M. Kropp in Uruguay. On May 9 the shape of the comet resembled a swallow with outspread wings. On May 10 there were two divergent tails. On May 11 a single slender straight tail. On May 18 two straight parallel tails. May 11 is the only day on which the photographs and the drawings show much resemblance.

Ast. Journ. No. 486 contains parabolic elements of Chase's Comet, 1898 VIII., deduced from observations 1898 Nov. 23, 899 Jan. 29, April 4, by Roger Sprague. The following are his elements:—

T	1898 Sept. 20·0738 G.M.T.	
i	22° 30′ 27″	1899·0.
☊	95 51 25	
ω	4 35 32	
log q......	0·35876	

The middle place is represented within 2″, showing that the deviation of the orbit from a parabola must be very slight.

Mr. Denning obtained some views of Tempel's Comet in July. It was pretty bright in a 10-inch telescope, and had a faint spreading tail pointing N.N.W. from the nucleus. A. C. D. C.

MINOR PLANET NOTES.—A planet, EP, of the 11th magnitude, was discovered by M. Mascart in Paris on August 26. This, however, proves not to be new, but identical with (32) Pomona.
A. C. D. C.

PROF. NEWCOMB ON THE SOLAR MOTION AND THE PARALLAX OF STARS OF DIFFERENT MAGNITUDES.—In No. 457 of the 'Astronomical Journal' Prof. Newcomb continues an investigation on this subject which he began some years ago, and which, as he points out, had been previously treated by Dr. Kapteyn. The geometry of the method of determining the solar motion may be briefly described. Suppose the solar system to be moving towards a point in the universe (the apex) with a velocity v, the Earth's distance from the Sun and a year being units, and let there be a star whose direction as seen from the Sun makes an angle λ with the direction of the apex. The velocity v may be resolved in the direction of the line of sight of this star, and at right angles to it. The latter component will be $v \sin \lambda$ at right angles to the line of sight in the plane containing that line and the apex. With the first of these components we have no further concern; but the second causes an apparent proper motion of the star, measured on the great circle joining the star and the apex, equal to $v . \pi . \sin \lambda$, if π be the star's parallax. Now if many stars are taken, and the determined proper motion of each be resolved into two components, σ on the great circle joining the star and the apex, and τ at right angles, each value of σ contains a quantity such as $v . \pi . \sin \lambda$; and on the assumption that the actual motions of the stars are distributed at random, the values of σ for stars whose parallax is known can be treated so as to give the quantity v.

The method of treatment which commends itself to Prof. Newcomb gives the value $v = 6{\cdot}4$, or a value for the solar motion of 30 km. per second, which is the same as the orbital motion of the Earth; but it is pointed out that as in this investigation stars which have *large* proper motions only were used, and the solar motion tends to increase proper motions in direction *from* the apex and to diminish those in the opposite direction, it may be that sufficient stars which are moving *towards* the apex were not included. Using therefore twenty-two bright stars in which there was no such bias, Prof. Newcomb finds

$$v=3{\cdot}5=16{\cdot}5 \text{ km. per second,}$$

which agrees with Kapteyn's conclusions from motions in the line of sight.

The paper is continued with brief discussions of the most probable value of the apex and of the mean parallax of stars of different magnitudes; the results are tabulated as follows:—

1. Kapteyn's determination of the absolute speed of the solar motion from Vogel's measures is very near the truth. In terms of the year and the Earth's mean distance as units, this is 3·5.
2. Kapteyn's formulas for the mean parallax of stars of any magnitude m, namely
$$\pi_m = k^m \pi_0, \quad \text{where} \quad k = \tfrac{1}{2}\sqrt{2},$$
is probably near the truth, at least up to the ninth magnitude.
3. For the brighter stars the mean parallax is probably less than is estimated. If we put $\pi_0 = 0''{\cdot}07$, we shall probably be as near the truth as we can get. So for stars of magnitude 2, $\pi_2 = 0''{\cdot}035$.
4. The most likely position of the solar apex is
$$\alpha = 277^\circ{\cdot}5, \quad \delta = +35^\circ,$$
but there is still an uncertainty of three or four degrees in the position.

THE PARALLAX OF THE STAR WB (2) XX. 1688 (= B.D. + 37°·4131).—Dr. Schur describes, in Ast. Nach. 3590, the results of a new determination of the parallax of 61 Cygni made at Göttingen in the autumn of 1897, the spring and autumn of 1898, and the spring of 1899. Four comparison stars were selected, whose distances from 61 in R.A. and Dec. were:—

	R.A.	Dec.		R.A.	Dec.
	s	′		s	′
a	−425	−42·8	*c*	−120	+39·3
b	+455	+42·5	*d*	+127	−42·4

a being the star whose name appears at the head of this note.

In the course of his investigation he found that the distance from *a* to *b* appeared to be subject to an annual variation of a parallactic kind, while that from *c* to *d* remained almost constant throughout the year. He concludes that the star *a* has itself a parallax of $0''\cdot6$, which is greater than the commonly accepted parallax of 61 Cygni, and only falls short of that of α Centauri.

This is an interesting conclusion, and at once suggested to the writer of this the inquiry whether the star a had any sensible proper motion.

The following places are brought up, with Struve's precessions, to the epoch 1900·0:—

Catalogue.	Date of observation.	R.A. 1900.	N. Dec. 1900.
		h m s	° ′ ″
Weisse's Bessel (2)	1825 (about)	20 55 20·76	37 32 27·0
Lund Zone 325	1880 Oct. 3	20·75	29·4
,, ,, 328	1880 Oct. 21	20·73	30·1
,, ,, 545	1892 Nov. 25	20·78	29·5

The declination in zone 325 has been increased by 10″ in accordance with a published erratum.

These observations seem to establish beyond doubt that the star's proper motion is very small, and hence that it is entirely unconnected with 61 Cygni, which has an annual proper motion of $+0^{s}\cdot344$ in R.A., $+3''\cdot24$ in Dec.

Now if the star a is really so close to our system as to have a parallax of $0''\cdot6$, the Sun's motion alone would produce a very sensible proper motion in this object *. Hence if this parallax be genuine, the only conclusion is that the star a is drifting in company with our own Sun, their motions being alike both in magnitude and direction. This would be a highly interesting conclusion, which the analogies of the five drifting Plough stars and some similar cases render not wholly improbable. But it would be premature as yet to regard it as established. Prof. Schur announces that he proposes to make a new investigation of the parallax of star a, choosing as comparison stars B.D. $+37^{\circ}\cdot4115$, $+37^{\circ}\cdot4159$, $+36^{\circ}\cdot4314$, $+38^{\circ}\cdot4325$. He invites others to take part in the research either by photography or heliometer measures. The result will be awaited with interest. A. C. D. C.

THE FALSE CROSS.—From two letters in our August number there appears to be some doubt as to the identity of the four stars which form the configuration known as the "False Cross." Mr. G. W. Nicolls, writing from Rio de Janeiro, and who may be therefore considered as acquainted with the Southern heavens, gives still another solution. He writes:—"What is known here as the 'False Cross' is formed by the stars κ, δ, ϵ, ι of the constellation Argo Navis." An anonymous correspondent at Cape Colony confirms this. He says that these four stars were pointed out to him as the "Mock Cross." "Where the Southern Cross is erect the Mock Cross is inclined. In it the stars are more equal in magnitude than in the Southern Cross, and as far as crosses in the sky go, it is not a bad one."

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* See previous note.—EDS.

PARALLAX OF THE ANDROMEDA NEBULA.—'Bulletin' No. 6 from the Yerkes Observatory, dated March 18 last, gives results of some measures made by Prof. Barnard with the 40-inch refractor, and conclusions drawn from them as to the distance of the great nebula in Andromeda from the solar system. The measures gave the position of the nebula with respect to two near stars, and were made in July and August 1898, and again in November and December. The difference in the measures at the two epochs is not more than might be due to error of observation, considering the difficulty of the observation, but the sign indicates that the nebula is farther away than the comparison stars. The author of the note, however, seems to prefer to think that the differences are accidental, and that no real parallax is to be detected, so that the distance of the nebula from the Earth is much greater than that of the nearest fixed star.

THE POSITION OF THE VISIBLE HORIZON.—In the course of a study of refractions and mirages ('Comptes Rendus,' July 31) at the surface of Lake Léman, some observations on the displacement of the apparent horizon were made by M. F. A. Forel. These observations were made at Morges in a laboratory on the border of the lake, where he could obtain a length of horizon of 16 kilometres. A small astronomical telescope mounted on a pier so that the horizon was 5 km. or 6 km. distant was used. The position of the true horizon was obtained by sights, both direct and with an artificial horizon. The angular differences between the true and apparent horizon varied from

$$\text{App.} - \text{True} = +501'' \quad \text{and} \quad \text{App.} - \text{True} = -272'',$$

or a total variation of 773″.

The factors causing this variation are: temperature of the air and of the water, the humidity and agitation of the air, the direction and quality of the wind, and atmospheric pressure, the most important being the difference of temperature between the air and the water of the lake. M. Forel has tabulated his values App. − True (elevation) according to the function $ta - te$, the difference between the temperature of the air surrounding the telescope and that of the lake, and is enabled to adopt the following approximate corrections from this cause:—

($ta - te$.)	(App. − True.)	($ta - te$.)	(App. − True.)
°	′	°	′
−6·5	−3	+0·5	+0·5
−4·5	−2	+1·5	+1
−2·5	−1	+3·5	+2
−0·5	−0·25	+5·5	+3

and to point out certain general rules, as:—

(1) The possible error in the position of the true horizon as deduced from observations of the apparent horizon is greater when the air is calm than when agitated.

(2) It is greater when $ta-te$ is positive (that is, when the air is warmer than the water).

(3) Since the observations are more uncertain when the air is calm and warmer than the water, morning observations are better than afternoon observations.

Double-Star Measures.—In Ast. Nach. 3584–5 Prof. Aitken gives a remarkably fine series of double-star measures made at Lick during 1898. The majority are difficult Burnham stars, and the observer is to be commended for the great amount of labour which must have been spent in procuring so many measures in one year. The list includes some stars of general interest and we give a few results here:—

β 883	37°·7	0″·26	1898·09
——	44·9	0·27	8·76
Sirius	161·9	4·22	8·73
Procyon	330·9	4·97	8·76
ε Hydræ	259·9	0·25	9·02
Σ 1728	8·7	0·19	8·31
ζ Herculis	289·6	0·45	9·75
κ Pegasi	294·5	0·16	8·50

In the same publication Prof. Hussey gives some of his measures of the companion of Sirius. The mean of this year is:—

Sirius	154°·6	4″·40	1899·28

The Summer of 1899.—Abnormal and long-continued droughts prevailed during the greater part of the summer, the rainfall in the months of June, July, and August amounting to 2·85 inches only, being nearly 4 inches less than the average fall for the preceding 58 years. One instance only of a similarly dry summer has been experienced in the years 1841 to 1898, viz. in the year 1864, when the rainfall for the three months (June to August) amounted to 2·50 inches, a slightly smaller amount than that for the present year. In the 94 days from May 26 to August 27, there were only 16 rainy days, falling principally between June 18 to July 3, and July 22 to 26, the more important droughts extending from May 26 to June 17, July 4 to 21, and July 27 to August 27. During these months there was an undue prevalence of easterly and northeasterly winds; a very small percentage of cloud, averaging 50 per cent. on the mean for the three months; and a very high proportion of sunshine, amounting to 242 hours in June, 248 hours in July, and 266 hours in August, giving a total amount for the three months of 756 hours, which is about 262 hours in excess of the average for the preceding ten years.

At times the weather was excessively hot, the maximum temperature in July having been 88°·5, the highest temperature recorded in July since 1896. In August the temperature reached 90° on the 15th, again closely approaching 90° on the 25th. On the average, a temperature of 90° may be looked for in August every eighth or ninth year: thus in 1859 August a temperature was experienced of 91°·3, of 90°·5 in August 1868, 93°·8 in 1876 (with three other days also exceeding 90°), 94°·2 in 1884, 95°·1 in 1893 (with two other days also above 90°), and 90° in 1899. The mean maximum temperature for the 53 days from July 6 to August 27 was 78°·3, the mean temperature for the period being 66°·6, or 4½° above the average. The mean temperature for the three months (June to August) in 1899 was 64°·0. This value has been exceeded on four occasions in the preceding 58 years, viz., in 1868, when the corresponding mean was 65°·1; in 1869, when it was 65°·0; in 1857, when it was 64°·4; and in 1846, when it was also 64°·4. The highest readings of the solar radiation thermometer were 158° on July 19, and 151° on August 10 and 25.

On account of the favourable weather conditions the August meteors were unusually well seen. W. C. N.

The Sun-spot Minimum.—During the months of August and September the Sun has been entirely free from spots, with the exception of about six days, on which there have been one or two very small faint spots of only two or three millionths in area. It will be remembered that the date of the last minimum was estimated as about 1889·6, and the last maximum 1893·9.

The Russian Calendar.—The 'Athenæum' says:—"We learn that the Russian Government propose to adopt the Gregorian Calendar at the beginning of next century—*i. e.* on January 1, 1901. According to this, they will still make next year a leap-year in accordance with the old Julian reckoning, but will afterwards fall into line with other Christian nations by making their dates correspond with what they would have been had the Gregorian rule of dropping a leap-year at the end of each century (except each fourth century) been observed since A.D. 325. Let us hope that before another century is completed the more simple and accurate rule will be substituted of dropping a leap-year at the end of each period of 128 years without exception, so that the next dropped after 1900 will be 2028."

Sir Howard Grubb's New Catalogue. — We have been fortunate enough to receive a copy of the illustrated catalogue recently issued by Sir Howard Grubb. This, though probably prepared for business purposes, is quite a text-book on the latest developments of astronomical instruments, containing, as it does, pictures of the best things turned out from the Dublin workshops —the Cambridge siderostatic equatorial, the twin equatorial as used by Dr. Huggins and Dr. Roberts; and of some not quite so recent—the Vienna 27-inch and the Melbourne reflector; and since it is easy to exhibit the capabilities of a photographic telescope, there are added specimens of astronomical photographs—nebulæ, star-clusters, &c.—taken with Sir Howard Grubb's masterpieces. The firm caters for all pockets: a light equatorial with a 4-inch glass can be bought for £52 10*s*.; a more solid instrument with a 6-inch glass costs £260; and there are grades intermediate to these; whilst if anyone wants an objective of 42 inches diameter, Sir Howard is prepared to make it for 10,000 sovereigns.

A LONG-ENDURING METEOR.—The following observations and remarks ('Comptes Rendus,' August 21) of M. C. André on a luminous meteor observed in the night of August 12–13 are very interesting. The observers, MM. Lagrula and Luizet, at 12^h 53^m Paris M.T., noted an especially luminous train to a meteor which was sufficiently bright to enable them to see surrounding objects. Although the path was rectilinear to the naked eye, it appeared sinuous and spiral when viewed with a magnifying-power of 5. At 12^h 55^m the aspect had changed; the light spread itself and presented an elliptic form elongated vertically (R.A. 130° with major axis towards 74° to 76° Dec.). At 12^h 56^m this ellipse had turned, the upper end inclined towards the west and the major axis horizontal, at the same time displaced to 138° R.A. and Dec. 75°. Commencing at 12^h 58^m the light continued to spread and was displaced towards the west, approaching the horizon, so that at 13^h 11^m its position was R.A. 143° and Dec. 72°. During all this time the object had the appearance, to the naked eye, of a small white cloud, similar in aspect to a portion of the Milky Way. No trace could be seen, either with the naked eye or in a small glass, at 13^h 13^m.

M. André remarks that the duration of 20^m is remarkable, but the successive changes of form and displacement show clearly that the cause of persistent luminous trains is due to the successive propagation of fragments into which they are separated by fracture.

POLARIS AS A MULTIPLE STAR.—An American correspondent telegraphs to the 'Standard' the results of some observations by Prof. Campbell at Lick. It seems that observations with the Mills spectroscope on the 36-inch equatorial show a displacement of some of the lines (the details are not yet to hand) in the spectrum of Polaris, from which Prof. Campbell concludes that the Pole Star is a spectroscopic binary, one component being dark and invisible.

It is said that the period of revolution is four days, and the binary system has a motion in the line of sight of 11½ km. per second towards the Earth. In 1896 Prof. Campbell measured this velocity as 20 km., and he attributes the change of rate to the presence of a second dark attracting body. We give these facts from the source mentioned above, and wait for accurate details.

DR. HUGGINS finds that in the spectra of stars characterized by strong helium lines, notably Bellatrix and Rigel, there is a dark line at the position of the strongest line of nitrogen at λ 3995. In Bellatrix he has also found lines corresponding with some other strong lines of nitrogen. So that he thinks there is strong probability that both nitrogen and oxygen are present.

THE comparison of observations with the ephemeris deduced from the orbits of ζ Herculis shows the need of obtaining as many measures of this binary as possible during the present summer. Dr. Doolittle has a short note to this effect in A. J. no. 466.

THE death, at the age of 88, of Prof. Robert Wilhelm Eberard Bunsen, which took place at Heidelberg on August 16, removes a notable personality from the scientific world. To the work of Bunsen in conjunction with Kirchhoff we owe the science of spectrum analysis, which has added new fields to the domain of astronomy.

ON September 14 last the astronomical effects, with other property, of the late Mr. Leeson Prince were offered for sale by auction. The large equatorial telescope of 6·8 inches aperture, made by Talley for the Royal Astronomical Society, described in vol. ii. of the Memoirs R.A.S., which formed one of the lots, was not sold. So that presumably this historic instrument is still in the market.

As we go to press information comes to hand of the discovery of a comet by M. Giacobini on September 29. R.A. $16^h\ 26^m\ 32^s$; N.P.D. 95° 10′; Daily Motion 2′ eastward and 10′ southward.

IT is announced that the new observatory and the great refractor at the Potsdam Astrophysical Observatory were formally inaugurated on August 26 last in the presence of the German Emperor.

OUR best congratulations to Mr. T. F. Claxton, Director of the Mauritius Observatory, on his marriage, which took place at Blackheath on September 14.

FROM AN OXFORD NOTE-BOOK.

THE appearance of the American Ephemeris for 1901 recalls attention to the decisions of the Paris Conference on Astronomical Constants and their effect. It will be remembered that a Conference of Superintendents of Ephemerides decided to alter the constant of aberration from Struve's value, 20″·445, to an adopted value, 20″·47; the constant of precession to a number to be determined by Prof. Newcomb; and to make other similar changes, all to commence with the volumes for 1901. The decisions raised considerable protest in America, and in England it has been pointed out that the date of the change is at any rate very inconvenient for those observatories where star catalogues are in course of construction. Thus at Greenwich the next Ten-Year Catalogue, epoch 1900, will be based on observations extending over the years 1897 to 1906 (presumably), and it is very awkward to have a change of system in the middle. It has therefore been deemed advisable in both the American and English Nautical Almanacs to give day numbers and ephemerides based on both the old constants and the new, at least for the present.

IN the volume for 1901 Prof. Harkness accordingly gives two sets of day numbers and two sets of star ephemerides, and many of us feel duly grateful to him for thus taking the lead in this concession. But the comparison of the two sets of numbers suggests one or two remarks. In the first place, one would expect no change in the numbers for aberration beyond the change of constant. Thus the Day Number D of the American Ephemeris (B of our own N. A.) represents $-20''\cdot4451 \sin \odot$ on the Struve-Peters system, and $-20''\cdot47 \sin \odot$ on the Paris-Conference system. Hence the values of log D should merely differ by $\log 20\cdot47 - \log 20\cdot4451$ or 0·00053; and if we restrict the logarithms to four figures, the differences might be sometimes 0·0006, but usually 0·0005. But a comparison of the two sets of numbers shows considerable fluctuations in the differences which, even when $\sin \odot$ is not small, range from 0·0004 to 0·0009; and when $\sin \odot$ is small show great irregularities. Apparently $\odot$ has not been taken from the same source in the two sets of tables, but from sources which may differ by half a minute of arc at least. I find no note in the letterpress to explain the difference. A similar remark applies to the C of the Am. Eph. (A of the N. A.). The comparison of the other pair of constants which involve precession and nutation is more difficult because the short-period terms are included in one set but not in the other.

THIS discrepancy, though worthy of notice in a work of such accuracy as a national ephemeris, is not in practice very important. But there is a point of a more serious character, on which I think a dictum of Prof. Harkness, and the results which follow it, are genuinely misleading. He says:—"For stars more distant than 11° 30′ from either pole, the apparent places derived by using the constants of the Paris Conference differ from those derived by using the constants of STRUVE and PETERS by quantities which never exceed $0^{s}\cdot015$ in R.A. and 0″·05 in Decl., and consequently throughout that region the star ephemerides given on pages 324 and 325 may be regarded as correct for either set of constants," so that the star ephemerides are only repeated for stars within 11° 30′ of the Poles. Now if these errors were purely accidental this would be quite correct; but seeing that they are systematic surely it is wrong in principle?

Consider, for instance, the value of 20″·47 sin ⊙ when sin ⊙ has the series of values 1·000, 0·951, 0·902, 0·853, 0·804, &c., &c., the common difference being the reciprocal of 20·47. The values are 20″·47, 19″·47, 18″·47, &c., which would appear in the star ephemerides given to 0″·1 only as 20″·5, 19″·5, 18″·5, &c. If for the Paris-Conference constant 20·47 we now substitute the old Struve constant 20·445, the values become 20″·445, 19″·445, 18″·445, &c., which would in the tables be written 20″·4, 19″·4, 18″·4, &c., differing systematically from the former by 0″·1. It is true that the number of cases where this difference of 0″·1 occurs is small compared with the whole number; but it is only by the careful recognition of such particular cases that we can distinguish between different values of the aberration constant. Differences in the adopted value less than 0″·1 can, of course, be recognized in tables which are only calculated to 0″·1, but only by care of the raised figure; on the other hand, to neglect such differences will introduce sensible errors into the mean of a number of observations, though the differences may appear trivial in individual cases. No one is more ready than myself to welcome the neglect of decimal places when the errors so made are accidental and disappear from the means; but systematic errors must be very carefully avoided or we shall have hopeless confusion. I would beg Prof. Harkness to reconsider this remark of his and see whether my objections are just or not. I am tempted to add a word or two on what might appear to be the other side—in favour of neglecting quantities for stars near the pole which are only apparently and not really sensible; but this might confuse the plain issue above raised and I therefore refrain for the present.

ALTHOUGH Science has little to do with political affairs, it would be affectation to refrain from an expression of regret at the iniquitous verdict of the Dreyfus Court-martial, if only because the single piece of evidence adduced in support of the accusation with any appearance of reality was tendered in the name of Science. The evidence of M. Bertillon as to the manner in which the *bordereau* was written ought to have been worthy of considerable attention, for he based his conclusions on measurements and coincidences which could be tested by anybody, much as a statement about a spectrum could be tested by anyone who would take the trouble to examine the coincidences of certain specified lines. Unfortunately there is no domain of scientific investigation in which an incompetent man may do so much harm as in this determination of coincidences, whether it be in spectrum analysis or in the detection of forgeries after M. Bertillon's method. The tool is keenly double-edged: in the hands of a master-workman it cuts into the unknown with amazing ease; but in the wrong hands it may do serious damage, as we have occasionally seen in spectroscopy, and as we have lately seen illustrated at the Rennes Court-martial. If M. Bertillon had been his father, who invented the system of identifying criminals, we might have had an examination of the writing of the *bordereau* which would give us correct information; but being, as he is, the least competent of his father's sons, who handles his father's tools without the ability to use them, he identifies the writing of the *bordereau* with a system of forgery "with mathematical certainty" as some writers say, there being merely the unsatisfactory circumstance that almost any other handwriting can be similarly proved to be a forgery of the same kind with equal certainty. For it was shown that if Dreyfus forged the *bordereau* on the system explained, he also forged continually in his ordinary office-work; and Esterhazy forged continually with the same key-word on the same system, and so did others. If the whole thing had not been so tragic we could afford to enjoy the exquisite comedy of M. Bertillon's explanations of these facts, viz., that, foreseeing detection of the forgery, Dreyfus and his family continually wrote in this way in order to be able to supply rebutting evidence of this kind, and also bribed Esterhazy to do the same!

SEVERAL correspondents, including "Screwtater" himself, have written concerning the lunar astronomy in *Jane Eyre* dealt with in last month's Notes, but there does not seem to be anything more wrong than applying the term half-moon to three-quarter moon (not *quarter* moon as suggested by a slip last month). There is no doubt that a half-moon cannot set so late as 5 A.M. in these latitudes either in January or at any other time of year; but the time of moon-set about this phase changes rapidly in January; and, as one correspondent puts it, "to set at about 5 A.M. the Moon would have to be about 10 or 11 days old; to call a Moon of this age a 'half-moon' does not appear to be a very serious crime for a novelist." I must confess, however, that on careful examination the Moon turns out to be rather big for a half-moon; I had looked at one or two figures rather too hastily; perhaps I may give an instance. In the *Companion* for 1894 the Moon sets on Jan. 17 at $17^h\ 7^m$, and the time of third quarter is Jan. $14^d\ 12^h$, while that of full moon is Jan. $21^d\ 3^h$. Reading the dates without much attention to the hours, the Moon is three days from third quarter and four days from full—nearer the third quarter than full (and hence falls into the class half-moon if we divide moons into "crescent," "half," and "full," say); but if attention is paid to the hours, the Moon is very near the "full" class. Several rather nice problems in elementary astronomy are suggested:—What is the latest time of day at which an exact half-moon sets in a given latitude? What is the smallest size of Moon that sets at six o'clock? And so on: even, perhaps—For how long a time may a Moon be considered crescent, "half"-moon, full moon?

It is well known that those who have had great experience in producing plays are often hopelessly wrong in forejudging what will touch a sympathetic chord in the public breast. Their masterpieces meet with no response, while a piece of rubbish (as they consider) runs a thousand nights. To compare small things with great, I find similar discrepancies between expectation and result in these Notes. For obvious reasons it is better to say nothing more about the failures; but the cases of success are sometimes curiously interesting. It should perhaps be remarked that one has rather scanty material to estimate success by; there is a tendency, perhaps, to attach too much importance to that outward and visible sign which results in a letter of reply; but if two or three people write, it often means that others who are actually silent "think a lot"; and so there is an excuse, if not a reason, for paying attention when a particular note produces two or three letters. That this was the case with Jane Eyre's astronomy need not perhaps cause surprise; but I was not prepared for four letters as a result of the casual reference last month to a rather cool request for photographs. Three of them were private letters, comparing similar experiences, which cannot or need not be further referred to; but the following from Mr. Chambers I am glad to reproduce:—

Dear Mr. Oxford Note-Book,—

I can readily beat the letter which you publish in the September *Observatory* at p. 352. Some ten years ago I received from an American "Professor" (his name shall be immortalized: it was Pritchard) a letter in substance as follows:—"Please arrange to let me have electrotypes of the following 50 engravings from your 'Handbook of Astronomy,' for which, of course, I will pay the usual electrotyping charges." I declined this request because I thought that to annex, in this fashion, 50 of my best woodcuts was asking a little too much. Thereupon I received a renewed application somewhat in the following very plaintive terms:—"I beg of you to reconsider your refusal about the 50 electrotypes, because I have written a book on Astronomy, and *have adapted my text to your woodcuts*, and if now I cannot obtain the woodcuts my manuscript will be wasted." I need hardly say that this additional plea furnished me with an additional reason for persisting in my supposed churlishness. The Professor had no doubt pirated my letterpress, but found it too costly to pirate the woodcuts, some of which had cost £5 apiece.

Eastbourne, Sept. 9, 1899. G. F. Chambers.

THE OBSERVATORY,

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The Expected Leonid Shower.

IT is scarcely necessary to remind the readers of this Magazine that this present November is the month when our Earth is expected to meet the thickest portion of the stream of Leonid meteors. The history and main points of the subject were exhaustively dealt with by Mr. Denning in volume xx. (1897), and enquirers may be referred also to more recent researches (with predictions) by Dr. Johnstone Stoney and Dr. Downing in our number of last May. In 'Nature' for October 19 Mr. Denning sums up the prospects for the present year in an article from which we take leave to quote the following:—

"We have been a little premature in our anticipations in recent years, in looking for the appearance of the meteors before the vanguard of the denser portion of the stream had begun to cross the Earth's path. There can, however, be no doubt as to the character of the ensuing display. The Earth will be sure to encounter one of the richest regions of the orbit at the middle of November, but whether or not this collision will occur at an hour perfectly suitable for its observation remains to be seen. It must be admitted that the exact time of the *rencontre* cannot be definitely stated. The materials upon which computations have to be based are not sufficiently numerous and consistent to enable exact deductions to be drawn from them. Morever, there is evidence to show that the system of meteors is constantly undergoing changes. The particles are spread out, and are still spreading out, over a very considerable section of the orbit, and are subject to perturbations by the larger planets. Different sections of the stream are affected unequally, so that the whole system, both as regards its conformation and distribution, suffers from such irregular disturbances that we must be prepared for the visible signs of developments of an unexpected character. In the present state of our knowledge it is impossible for us to allow for all the various circumstances and conditions which control the visible aspect of the shower, from year to year, and modify its orbital elements.

"Calculations which have been made independently by several authorities show that the influence of Jupiter and Saturn, since the last return of the shower in 1866, has been exerted in displacing the node, so that the phenomenon may be expected a day late in the present year. It will probably occur just before sunrise on November 16. Drs. Stoney and Downing, in a paper published in the 'Proceedings' of the Royal Society, vol. lxiv. p. 406, state that a noteworthy outcome of their investigations is that the meteor-group which gave rise to the display in 1866, made a near approach to Saturn in 1870, and to Jupiter in 1898. On the latter occasion the meteor-cloud was distant from Jupiter by an interval of space less than that separating the Earth and the Sun. Berberich (Ast. Nach. 3526) has also discussed the orbit-perturbations of the Leonid stream, and concludes that the meteors will appear about a day later than they would have done under normal conditions. If there had been the average annual displacement of the node (equal 102″·6) the recurrence of the shower might have been anticipated on November 15 at about 1 A.M., but the perturbations seem to have increased the longitude of the node to the extent of 1¼°; so that the greatest intensity of the display must be awaited on the morning of November 16, in the twilight preceding sunrise.

"But it must be admitted that these deductions are liable to some uncertainty. Last year the predicted late-coming of the meteors was far from being corroborated by observation. The maximum number of meteors was recorded on the morning of November 15, and very few Leonids were presented on the following morning, though computation had indicated the latter as the time of maximum. In view of the prevailing doubts there seems no alternative but to watch for the shower throughout the morning of the 15th, and failing its brilliant apparition then, to repeat the watch on the morning of the 16th. The maximum may be displayed at any time between November 15, $0^h\ 30^m$ A.M., and November 16, $6^h\ 30^m$ A.M.

"In England a November sky is cloudy on at least three nights out of four, and this year we shall have moonlight to consider as well, for our satellite will be nearly full, and must largely detract from the striking character of the display. Should the meteors appear on the morning of the 15th, they might, however, be seen on a dark sky, for the moon will set about 2½ hours before sunrise. The Leonids are fine meteors; a large proportion of them are as bright as 1st mag. stars, and, notwithstanding moonlight, will create a conspicuous effect if they return in great numbers. On the occasion of the last grand display on the morning of November 14, 1866, the writer was much struck with the number of tolerably bright meteors, and observed several which were many times brighter than Venus at her best. These Leonid fireballs gave lightning-like flashes, and left short green streaks, enduring for five, ten, fifteen minutes, and even more. The approaching display will be sure to supply a few of these splendid objects.

"At every station where the weather enables the shower to be successfully witnessed, certain features ought to be particularly recorded. The meteors should be counted and the time of maximum ascertained. It will be useful also to determine the hourly rate of apparition by noting at certain regular intervals the number which appear. By counting during short intervals and continuing the work for several hours, the rise and fall of the display as well as the number per minute at and near the time of maximum might possibly be obtained. In the event of an exceedingly abundant display, similar to that seen in America in 1833, the observer may feel bewildered and find it impossible to record the exact numbers. In such a case the figures should be estimated as carefully as possible."

The Spectrum of β Cygni.

THE spectra of double stars form such an arduous branch of enquiry that there is little reason to be surprised at the small amount of attention bestowed upon it. Investigators are apt to look askance at subjects certain to prove highly exacting of pains and patience, and uncertain in their yield of results. Sir William Huggins, however, executed a reconnaissance in that direction as part of his initial campaign of 1863–64, and derived from it the important lesson that the colours of the stars depend upon the nature of their investing atmospheres. But the couples examined by him—β Cygni and α Herculis—were among the very few in the heavens accessible, to any purpose, with the instrumental means then available; and, pending the realization of very material improvements in them, there was nothing further to be done or usefully attempted along the line struck out.

A third of a century elapsed before an effective advance could be made; and, most remarkably, it was the original pioneer who again came to the front. The intervening development of spectrography, together with the completion of the Tulse Hill reflecting-slit, gave the needed facilities, and at last, in 1897, a fresh start was made with the analytic self-registration of the contrasted stars of β Cygni on plates exposed by Sir William and Lady Huggins. The spectra gave evidence of a signal diversity in constitution. The hydrogen-series was, on that of the fifth-magnitude "sapphire" component, writ large and strong; it appeared with no less emphasis, relatively to brightness, than in Vega; while the golden primary, to which we may, for convenience, appropriate the Arabic name "Albireo," showed itself as of pronounced solar type. That the small star should have progressed more slowly than the large on the evolutionary track laid down by the best authorities, is assuredly perplexing; yet the same relation seems to be almost universally prevalent between tinted double stars, and doubtless has its roots deep down in the history of stellar systems. Of its implications as regards stellar generic growth, much might be said and surmised; but they are too wide for summary discussion. A somewhat less recondite topic is, however, suggested by the obvious circumstance that the photographed spectra do not explain the vivid colouring of β Cygni; they might have been taken (broadly speaking) from any two stars in the sky of the second and first types respectively. Nothing else, indeed, was to be expected, since the special absorption differentiating them from the common run of stars was known to lie outside the range of sensitiveness of ordinary plates. In the case of the blue member of the pair, at any rate, this had been virtually ascertained. A set of dark bands, cutting out a goodly proportion of its yellow and orange rays, were observed by Sir William Huggins in 1864, and again by Dr. Vogel in 1872; and they directly correspond with, and fully account for, its chromatic peculiarity. This is shared, we need hardly remind our readers, only by compound objects, no solitary star "dartling" genuine azure light; so that the bands in the yellow of the companion of Albireo are not unlikely to prove distinctive of dependent stellar globes. Hence their lasting record by means of isochromatic plates will be of great interest; while their recognition in the laboratory would dispel much of our present ignorance regarding the chemistry of colour in stars.

The "topaz" hue of Albireo itself seems to be of somewhat indefinite production. We see in it, perhaps, the general result of a multitude of particular causes, modifications in the relative conspicuousness of line-groupings, and the like; or it may be simply an enhancement of that "veiling" effect to which sunshine owes its primrose tinge. But, although the spectrum naturally includes everything that affects colour, the signs may be present in a scarcely legible form. It is, however, otherwise noteworthy.

In the course of her discussion of the Draper Memorial spectrographs (1888–1896), Miss Maury drew up a list of eighteen stars of "composite" type*. "In spectra of this class," she says, "the K-line appears either unduly narrow, or overspread with a peculiar haziness. This appears to be due to the presence of an additional star, having a spectrum which belongs to some group earlier in the series. It is also significant that in such spectra the first-type characteristics predominate in the ultra-violet, the second- or third-type features in the green and blue" †. These duplex effects demonstrably own, in certain of the instances enumerated, a duplex cause. For they include γ Andromedæ, ε Boötis, and 31 Cygni, all three remarkable pairs, close enough to give, in small-scale delineations, superposed spectra. The light of the adjacent components accordingly *ran together* on the Harvard plates, and thus stamped upon them the observed hybrid prints. There is then good ground for the suspicion that composite rays really emanate from a compound source, even where visual proof of duplicity is absent. Among these *crypto-doubles* are *o* Leonis and Albireo. Miss Maury's description of the spectrum of the former, as probably derived from a star resembling Procyon blended with a star a trifle later than Sirius, was curiously and unexpectedly ratified in the course of Professor Campbell's line-of-sight work with the Mills apparatus on the thirty-six-inch refractor. He detected in *o* Leonis a spectroscopic binary with a period of 14½ days, and a spectrum thought to be compounded from two distinct light-sources. The doubling of the lines does not appear to be so obvious as in β Aurigæ and a few analogous objects; nor could it be, if the conjoined spectra are, as alleged, substantially different, since in that case the absorption-elements of each would not be accurately matched. Further details respecting this exceptional system will doubtless ere long be at hand; and it will be important to compare the relative mass and luminosity of its components with individual differences in the quality of their respective emissions.

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* 'Harvard Annals,' vol. xxviii. p. 93.

† *Ibid.* p. 11.

The same kind of experiments remain to be tried on Albireo. Their result, however, depends not merely upon the nature of the star as it is in itself, but upon an extraneous circumstance. Even if it be actually a close pair, their revolutions may be carried on in a plane so highly inclined to the visual ray as to be without effect in line-displacement. In which case the spectroscope would ignore them, and their reality should continue a matter of surmise. Should it, on the other hand, be attested by the customary proofs, we shall have a third example of invisible duplicity in the chief member of a wide visible couple, the others being Polaris and Mizar (ζ Ursæ Majoris). And the juxtaposition, in Albireo, of dissimilar spectral types should render it the most instructive of all. But as a spectroscopic binary it still only looms upon the horizon of discovery.

A. M Clerke.

A Splendid Meteor.

I have received several accounts of a fireball which appeared on the evening of September 8, and give extracts from a few of the descriptions:—

Sept. 8... $8^h\ 21^m$. Fine meteor observed. At first it was only about 3rd mag., but increased until it was much brighter than Venus. The nucleus flashed out brilliantly about 4° before the end-point. Its path was from 313° $+4°$ to 287° $+4°$, which it traversed with a slow motion, the duration being about $2\frac{3}{4}$ seconds. The colour was bluish white, and there was a slight train.—T. H. Astbury, Wallingford.

Sept. 8... $8^h\ 22^m$ to $8^h\ 26^m$. The sky was partly cloudy, and the meteor appeared to be stellar at first, and only $1\frac{1}{2}$ or 2 mag. It passed under μ and ϵ Aquarii at a little greater distance than that between β–η Pegasi, and was directed from 1° above γ–θ Piscium. Approximate path from 321° $-12°$ to 281° $-28°$. It ended in a kind of explosion, covering about $1\frac{1}{2}$ times the apparent visual area of the Moon. The meteor was probably very brilliant at this point, as it was behind clouds. Duration about 4 seconds.—A. King, Leicester.

Sept. 8.... $8^h\ 24^m$. An observer in Handsworth Park, Birmingham, describes the meteor as a splendid fireball of a bluish-green colour, brighter than Jupiter. Its motion was slow, and while in sight it travelled from S.E. to S.W.

A comparison of the various observations shows the radiant to have been at 347° $+3°$ (near γ Piscium). The meteor appeared over the English Channel (lat. $50\frac{1}{2}°$, long. 1° E.), at a height of 71 miles, and disappeared over Alresford, Hants, when it had descended to 26 miles. Its length of path was 114 miles, and velocity approximately 20 miles per second. Had the meteor reached the Earth's surface, it would have fallen at Bristol, but it was about 67 miles from that city at the time of its disruption.

The radiant at 347° $+3°$ agrees with that of a well-defined meteoric shower often observed in the month of September. There is little doubt that it is also connected with the same system as that which furnished the fireball of 1875 Sept. 14, for which Lieut.-Col. Tupman computed a radiant at 348° $\pm 0°$. This shower is No. cclxviii. of my "General Catalogue of Meteoric Radiants" (Memoirs R. A. S. vol. liii.). Summarizing the various observations of this September shower, we have the following:—

Date.	Radiant.	Meteors.	Observer.
1886 Aug. 24–Sept. 7......	346° +1°	5	W. F. D.
1885 Sept. 1–4..............	346 ±0	9	W. F. D.
Sept. 3–14	346 +3	......	Schmidt.
1899 Sept. 8	347 +3	Fireball	W. F. D.
1875 Sept. 14	348 ±0	Fireball	Tupman.
1876–89 Sept. 15–20	346 ±0	10	W. F. D.
Sept.	344 ±0	......	Schmidt.
Sept.	346 −3	......	Heis-Neumayer.
1885 Sept. 17	345 ±0	4	W. F. D.
1898 Sept. 17	343 ±0	Shooting-star	W. F. D.

There is radiation from this point in earlier months and also near the end of September.

Bristol, 1899, Oct. 19. W. F. Denning.

Modern Geodesy in France *.

GEODESIC science, which had its cradle in France and developed so freely in our country during the 17th and 18th centuries under the influence of memorable expeditions organized by the Academy, and which still flourished at the beginning of this, when our geographical engineers were covering our territory with a continuous triangulation, found itself almost abandoned when that work was completed. The network having been brought to a close, enterprise seemed to be given up. The Dépôt de la Guerre still contained some geodesists, but it gave no food for their activity. One may say that with 1830, the date of the fusion of the Ingénieurs Géographes into the Corps d'État-Major, began a period of real weakness in the history of our geodesy.

At the same time, abroad, the geodesic movement took a considerable flight. In England with Airy, in Germany particularly, under the influence of Gauss and Bessel, geodesy made remarkable progress. Methods improved, instruments were made more accurate, observations became more precise, results more certain. Instructed in our school, our rivals had already far out-distanced us; they did not hesitate to declare that our operations were not up to the level of modern science, and they showed the decadence of those works that we had pursued for two centuries.

It is certain, to speak only of the work of the Carte, that the French triangulation was open to well-founded criticism. It had been known for a long time that the meridian of Delambre and Mechain, upon which all our network hung, contained some grave imperfections. The ingénieurs géographes had discovered and corrected several of them, but they had not disposed of them all; they themselves had supplied meridian arcs and parallels which were not without fault. The network of France, the first in the history of State triangulations, is a work admirable considering the time at which it was made, but a work to be revised if it is to take a scientific place.

This was the state of things when, in 1866, England asked that a trigonometric junction should be made with France by way of the Calais Straits. It was with great trouble that the Dépôt de la Guerre were able to induce young officers d'État-Major to attach themselves to the remnants of the Ingénieurs Géographes, with the view of forming a brigade to collaborate with the English geodesists. In this geodesic tourney our representatives had the bitterness of ascertaining the great superiority of our neighbours as to equipment, organization, and methods. They had adopted long since the method of duplication, and had at their disposal large circles of Ramsden furnished with microscopes for reading the limb: with us, on the contrary, the method of the repeating circle was still followed, and the instruments employed had only the circles, old and worn, which had been used for the triangulation of the Carte.

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* **Abridged translation of part of a Memoir by Col. Bassot in the 'Annuaire of the Bureau des Longitudes, 1899.**

This circumstance was not without its influence upon the future of French geodesy. One of our officers, Capt. Perrier, brought from this campaign the desire for revenge. He resolved to consecrate his career to geodesic work, that he would be able some day to put in a place of honour in the army a service for a long time misunderstood, and to restore a science that our country had so unfortunately abandoned. In the campaigns which followed, in Corsica, in Algeria, he ripened his ideas, but he had already conceived vast projects capable of restoring to the French network the precision which was wanting to it.

It was at this epoch that the Bureau des Longitudes and even the Observatory recognized the inferiority of our triangulation, and asked for a revision of the meridian of Delambre and Mechain. Pressed by the opinion of scientific men, the Dépôt de la Guerre understood the necessity of this revision, but declared that it was not able at once to undertake the work immediately, having neither the pecuniary resources nor a staff fitted for operations of precision. An important discovery allowed both these obstacles to be overcome. Stationed upon the mountains of the province of Oran, Perrier had the good fortune to notice under special atmospheric circumstances the visibility of some of the summits of the Sierra Nevada in Spain, so that it was possible to connect geodesically Algeria and Spain. Besides, the meridian of France already welded to the English triangulation would be able to cross over Spain, and to be prolonged into Africa as far as the desert, stretching, therefore, from the Shetland Isles to Laghouat. This magnificent project, imagined by Biot and Arago when they prolonged the meridian of Barcelona to Formentera, would become practically realized. The Bureau des Longitudes was struck by the scientific interest which was attached to such an enterprise. In an official note, M. Faye, President of the Bureau, pointed out to Marshal Niel, then Minister of War, that the moment had come for France to renew ancient traditions, and that it would be to its honour and profit to carry out a work of such moment to science. Thanks to the strong influence of the Bureau, the project was adopted in April 1869, and Perrier was charged to begin the meridian of 1870; later on, as far as circumstances would permit, the Spanish-Algerian junction would be effected to continue the meridian of Algiers to Laghouat.

This is the programme which has been realized since 1870, and which forms the contribution of France to modern geodesy. To this must be added the regular triangulation of Algeria, which was begun in 1860, and has been carried out during the same period, the survey extending to Tunis. Thus our country has taken an honourable place in the geodesic movement which characterizes the epoch. It is just to ascribe the merit to Perrier, who was the promoter of all the work, as well as to the Bureau des Longitudes, who encouraged and guided him in his enterprises.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Robert Hooke and his Astronomical Discoveries.

GENTLEMEN,—

Of Robert Hooke, Grant well observes that he "would be well entitled by his genius to occupy a high place in the history of physical science, if he had displayed more uprightness and moderation in his relations with contemporary philosophers." I do not propose to enter here into the vexed question of his claim to have anticipated Newton in the discovery of the law of gravitation; whatever the former and others may have suggested, to the latter indisputably belongs the glory of having worked it out mathematically. But I was led, by the report of a note read by Mr. A. Brothers at the May meeting of the North-western Branch of the British Astronomical Association, and printed in vol. ix. no. 3 of their 'Journal,' to look into a claim made on Hooke's behalf of having anticipated Halley in the idea that some comets return periodically. This is founded on an account of a lecture given by Hooke at Gresham College, and described by Pepys under date March 1, 1665, in his famous Diary. He there says that Hooke stated that it was very probable that the comet of 1664 was identical with that of 1618; and though we now know that such was not the case, but that the orbits of both these comets are undistinguishable from parabolas, Mr. Brothers suggests that "knowledge derived from Hooke's lecture may have suggested to Halley the investigations which have added fame to his name as the first to predict the periodical return of comets." Before accepting this, it would be well to refer to Hooke's own words on the matter as given, not in a report of his lecture, but in his published work, 'Cometa,' which appeared in 1678. He there says: "And 'tis not impossible that the Comet of 1618 might be the same with this [of 1664] if we suppose the Nodes of it to have a motion contrary to the order of Signs: and that the same Node which in this Comet, according to this supposition was in Gemini, was then about Scorpio or Sagittarius: but these are conjectures I shall not insist on, because neither in this nor in that have we Observations sufficiently accurate to build any Theory upon." When Halley made his celebrated investigation into the orbits of all the comets which had been then observed with sufficient accuracy to make this possible, resulting in the prediction of the return of the comet of 1682 in 1758 or 1759, Newton had proved that comets, as well as planets, moved in conic sections round the Sun, and therefore it was a natural idea that some of the former might revolve in elongated ellipses and return at long intervals, whilst others might simply describe a parabolic or hyperbolic curve about the Sun and never return.

With regard to Hooke's supposed discovery of the parallax of γ Draconis, he evidently fell into the same mistake which many others did, that changes of stellar apparent place due to aberration were caused by parallax—the true source of these being discovered more than fifty years afterwards by Bradley. I alluded last month to the fact that Picard was the first to point out that the changes were not parallactic. Those which he noticed and discussed were of the Pole Star, observed by him at Uraniborg in 1671, of which he remarks (p. 19), "Mais sans vous arrester à des Phénomènes qui peuvent avoir eû des causes extraordinaires...."

Hooke seems to have noticed, at least as early as any one else, motions of spots on Mars and Jupiter, which proved the axial rotations of these planets, though Cassini was the first to determine the durations with some accuracy.

Before closing this letter, I wish to draw attention to a matter in which Hooke apparently really had decided priority, but, although Sir W. Herschel wrote much about it afterwards, it is very far indeed now from being an established fact. I mean the suggestion of a connexion between solar spots and terrestrial heat. In a paragraph on "Maculæ in Sole" contained in his 'Lampas,' which appeared in 1677, he speaks of large spots seen on the Sun about the end of July in that year, at which time great heat was experienced over not England only, but generally in Europe; but he adds the heat was still very great in August after the spots had disappeared on the 4th of that month: "it may therefore possibly be that other parts of the body of the Sun may have an extraordinary inflammation, which may cause so fervent and lasting heats to have happened this Summer." The year in question was 222 years ago.

Yours faithfully,

Blackheath, 1899, Sept. 1. W. T. LYNN.

The Observation of Early Leonids.

GENTLEMEN,—

It seems desirable that meteoric observers should look out for early Leonids in the second week of November, for there is strong reason to believe that the shower begins on November 7. It is certainly very feeble then, but a watch maintained during the morning hours will be sure to reveal a few meteors of the Leonid type, with flights directed from the usual radiant in the sickle of Leo. This year the nights between November 7 and 12 will be more favourable for meteoric observation than at the middle of the month, when moonlight will offer a serious hindrance. It would be well if the regular observers in this department maintained simultaneous watches of the firmament on the mornings from November 8 to 13, so that in case any bright Leonids made their appearance they might be suitably recorded at several places, and their presumed identity with the major shower fully investigated. In November there are a considerable number of radiants of swift, streak-leaving meteors in Leo and in the immediate region of that constellation, so that it is not easy to discriminate between the true Leonids and meteors supplied by adjoining streams. Though a meteor may be precisely conformable in its direction of flight and general aspect with that of a true Leonid, it may nevertheless belong to an entirely different system. In order to obviate mistakes in attributing meteors to their radiants, it is very necessary to obtain duplicate or multiple observations of the individual paths, for these, if correct, will indicate the point of divergence beyond all question.

Of course, only habitual observers in this line will care to stay out " in the small hours " of cold November mornings, watching a sky in which not more than about fifteen meteors appear within an hour. We can only expect ordinary persons to undertake vigils at a time when a really brilliant shower is due, and when meteors may fall at the rate of 1500, or just possibly 15,000, in an hour, as on the morning of November 15 or 16 next. It is different with scientific observers, who, while thoroughly appreciating the spectacle offered by a fine display, will study the least interesting features of the phenomenon equally with the most attractive ones.

Reliable observations of outlying Leonids visible on a few nights preceding the main group will afford useful evidence not only in regard to the duration of the shower, but also upon the position of the radiant. A few accurately recorded tracks of such Leonids as moved in declination, either to N. or S. of the centre, would clearly prove whether the radiant remained fixed or travelled slowly eastwards about one degree per day, in accordance with the theoretical views of the late Dr. Kleiber.

It is curious that the place of the radiant, as determined by the photographic method, seems about 2° east of the positions ascertained by eye-observations of meteor-paths. This is, however, partly explained by the effects of precession. Some of the observers, in deriving the radiant, employed old celestial globes or star maps, and gave the places without bringing them up to date. But this can scarcely explain the whole difference, which is larger than one would have thought probable, the mean of a large number of eye-estimated radiants being $149^\circ\!\cdot 7 \; +22^\circ\!\cdot 7$, while a few photographic values give $151^\circ\!\cdot 7 \; +22^\circ\!\cdot 1$.

Yours faithfully,
Bristol, 1899, Oct. 19. W. F. Denning.

The Solar Motion.

Gentlemen,—

It is with great diffidence I make any comments on the article quoted on p. 373, entitled " Professor Newcomb on the Solar Motion," and yet it seems to me that both here and in most other statements with reference to the " solar apex " there is an important omission. Result 1, as given on p. 374, gives what is stated to be "the absolute speed of the solar motion," whereas should there not be added the words "with reference to —— "? A similar addition should be made to Result 4. Different calculators of the solar motion use different sets of stars as a basis of their calculation; and seeing that all stars are believed to be in motion, and the whole group used may have a motion of its own, it is clear the solar motion is only a relative one for this particular group of stars, and it is therefore necessary, in the statement of the result, to state what the group is. No doubt the varied results arrived at by different calculators largely arise from the varied sets of stars used in the calculations, and therefore do not indicate any error in calculation or hypothesis.

Is the uncertainty of 3° or 4° mentioned in the fourth result simply a statement of the probable error of Prof. Newcomb's determination, or does it refer to the difference from the results obtained by others? In the latter case it is no real question of uncertainty, for the result may be quite accurate as regards the particular problem attacked by Prof. Newcomb.

Surely we could only talk of the "absolute speed of the solar motion" if we had some fixed point to compare it with; whereas we know of no such point in the universe. Do we know that even the luminiferous ether is stationary?

West Hendon House, Sunderland,
1899, Oct. 10.

Yours faithfully,
T. W. Backhouse.

Leap-Years.

Gentlemen,—

I do not think that many people could be found to admit that the proposed rule for leap-year, in your quotation on p. 378, is a "more simple and accurate" one than the present. Whether or not it is more accurate, it is surely nothing like so simple. The present rule is sufficiently easy to remember, but how many people would be able to remember what leap-years were dropped if it were done every 128 years? Then, if the statement on p. 378 is correct, how does it happen that the present difference between the Greek Church and the rest of Christendom amounts to 12 days, instead of only 11 as would appear from the statement?

West Hendon House, Sunderland,
1899, Oct. 21.

Yours faithfully,
T. W. Backhouse.

The Solar Eclipse of A.D. 733.

Gentlemen,—

Humboldt, in his 'Cosmos' (vol. iv. p. 334 of the English translation by Otté), says of this year in a note:—"One year after the Arabs had been driven back across the Pyrenees after the battle of Tours [by Charles Martel, A.D. 732] the Sun was so much darkened on the 19th of August as to excite universal terror (Schnurrer, 'Chronik der Seuchen,' part i. p. 164)."

Mr. Johnson and Mr. Chambers (following him) have concluded that these words refer to an annular eclipse of the Sun which is mentioned in the Anglo-Saxon Chronicle. But if so, it would be necessary to suppose that the date given above is five days in error, as the eclipse occurred on the 14th of August.

I had the curiosity to refer to Schnurrer to ascertain his own view of the matter, which is expressed as follows. After speaking of the battle of Tours in A.D. 732, he says:—"Im nächsten Jahre wurde am 19. August die Sonne auf eine Schreken erregenden Weise verdunkelt; es scheint keine Verfinsterung durch den Mond, sondern eher durch meteorische Substanzen gewesen zu seyn; auch der Mond wurde am 31. Januar im Jahre 734 zuerst blutroth und dann dunkel-schwarz gefärbt. Beydes berichtet Bäda."

As will be seen presently, there is no need to attach much weight to his opinion that the darkness in the first phenomenon was caused by a swarm of meteors passing between us and the Sun, however we may be struck by the fact that the date given is only a few days after that when the Perseids were due. But we can hardly doubt that the second phenomenon was an eclipse of the Moon, though the date given is a week too late, as a lunar eclipse really did take place on the 24th of January, A.D. 734.

We must demur to the concluding sentence, "Beydes berichtet Bäda." Beda's history finishes in A.D. 731. Apparently this fact was no stumbling-block to Schnurrer, for he goes on in the next paragraph to say:—"Von dem Jahre 737 bis 741 scheint nach den Aeusserungen von Bäda auch in den nördlichen Gegenden grosse Trokenheit geherrscht zu haben."

Now Bede died in A.D. 735 * and could have described neither drought nor anything else in the above years. Schnurrer rightly quotes him as speaking of a comet seen in the year 728. The Anglo-Saxon Chronicle mentions that and the eclipse of the Sun in 733 and of the Moon in 734, but without giving their dates. Simeon of Durham, however ('De gestis Regum Anglorum'), gives "xix Kal. Septemb." for the former, and "ii Kal. Februarii" for the latter, dates corresponding to August 14 and January 31. Now the former of these is the real date of the solar eclipse in A.D. 733, and renders unnecessary Schnurrer's conjecture about a meteoric display, whilst the latter is as quoted by him, and must be a week too late.

Yours faithfully,
W. T. Lynn.

Blackheath, 1899, Oct. 4.

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* The Anglo-Saxon Chronicle mentions it under 734, but the true date was in all probability 735, which is given by Simeon of Durham.

OBSERVATORIES.

[Continued from p. 370.]

HEIDELBERG. *Max Wolf.*—The establishment of the Grand Ducal Observatory was begun in 1897 and the instruments are not yet completed. The principal telescope is to be a photographic refractor, presented by Miss Bruce. There is also a 6-inch refractor with a triple object-glass and a 2½-inch transit. The Observatory was formally opened on June 20 by the Grand Duke and State Officials. By the end of 1898 regular astrographic work with the triple O.G. was begun and some 150 plates exposed. Naturally the greater portion of the time was occupied in testing the instruments. Attention has been given to observation of small planets, photographs of comets, and also to the zodiacal light. The Observatory is also a meteorological station.

HEIDELBERG. *Max Wolf.*—The private observatory of Dr. Max Wolf has been merged into the State Observatory, the instruments and transit-hut being transferred.

JENA. *Otto Knopf.*—The Jena object-glass (174 mm.) set up in 1896 shows as yet no deterioration—12 mag. planets can be seen with it. Observations of Comets 1898 I., IV., IX., and of small planets. Transit employed in latitude observations according to Talcott's method. Meteorology.

KALOCSA. *T. Fenyi.*—The Sun's limb was examined on 181 days, and on 17 occasions prominences exceeding 100″ in height were observed, the highest being 237″ on Oct. 27. Great attention to sun-spots, of which numerous drawings were made. Spectroscopic observations on spot groups. Meteorology.

KASAN. *D. Dubiago.*—Dr. B. von Engelhardt having presented his instruments and library to the Kasan Observatory, the buildings required expansion. It was thought better to erect a new observatory, affiliated to the University, and hopes are entertained of beginning work by the end of 1899. The instruments presented by Dr. Engelhardt are those he used at Dresden, viz., a 12-inch refractor, comet-finders, two transits, two clocks, &c. The publications are to be, as before, in the Russian language only.

KIEL. *Paul Harzer.*—The instruments are being thoroughly overhauled and renovated and the rooms re-arranged. A small wooden structure has been erected, wherein students may use small instruments.

KIEL. *H. Kreutz.*—Dr. Kreutz presides over the 'Centralstelle für Astronomische Telegramme' and the 'Astronomische Nachrichten.' Computations.

KÖNIGSBERG. *H. Struve.*—The meridian work consists of Sun observations, of planets, Burnham double stars, and fundamental stars. The new 13-inch refractor came into use in March, and Dr. Cohn has measured some double stars. The heliometer has been devoted to parallax observations.

LEIPZIG. *H. Bruns.*—Experiments made with heliometer by Dr. Peters on the effect of atmospheric dispersion on distance measures; parallax of σ Draconis; lunar diameters. Dr. Hayn continued his triangulation of the lunar surface with the refractor; places of comets; Eros, &c.

LUND. *C. V. Charlier.*—Researches on the effect of magnitudes in star-transits; zone observations (35° to 40°); examination of proper motions.

MILANO. *G. V. Schiaparelli.*—The 18-inch refractor employed for measurement of double stars, more especially the Burnham pairs; the 8-inch was also used for this purpose by Prof. Celoria. Geodetic work.

MÜNCHEN. *H. Seeliger.*—The Repsold meridian-circle was in constant use, and 3217 observations of 694 stars secured by Dr. Oertel. The 10½-inch refractor was used by Dr. Villiger, principally for planetary and comet work, 217 observations of 31 planets being made. The comets observed are:—Perrine 1898 I.; Wolf 1898 IV.; Giacobini 1898 V.; Coddington-Pauly 1898 VII.; Perrine-Chartordet 1898 IX.; Brooks 1898 X. Dr. Villiger also made measures of Saturn, and estimations of brightness of Nova Aurigæ.

POTSDAM. *H. C. Vogel.* (Astrophysical Observatory.)—Miss Everett left for Vassar College Observatory, her place being filled by Dr. Ludendorff. Continued researches on the spectra of stars expected to be in Class I. It is found that of the 528 stars whose spectra have been examined, 453 come into Class I. From researches on the spectra of α Aquilæ it would appear that it is really a triple star. Prof. Lohse made several drawings of Jupiter; the great red spot was not well seen. Professors Müller and Kempf continued their photometric Durchmusterung of the northern sky within the zone +20° to +40° Dec., 4416 stars being examined. The probable error of the determinations, both of magnitude and colour, have turned out to be very small. Two variables of short period have been discovered, viz., U Vulpeculæ and SU Cygni. Prof. Lohse obtained Sun-pictures (10 cm. diameter) on 104 days. Work on the photographic chart was maintained under Prof. Scheiner, and by the end of the year 903 plates had been taken. Miss Everett had measured some 12,000 stars before July, and Dr. Ludendorff afterwards added about 6000. The reduction of the measures is kept up. Time service.

STRASBURG. *E. Becker.*—The 18-inch refractor was employed on Comets Perrine 1898 III., Coddington, Giacobini, Wolf, Perrine 1898 VI., Perrine 1898 IX., Brooks, Chase; small planets Monarchia and Eros; and occultations by the Moon. The work with the meridian circle consisted of a large number of observations of α and δ Ursæ Minoris, 2022 circumpolar stars, 1834 fundamental stars, and major planets. Latitude observations with altazimuth.

STOCKHOLM. *Karl Bohlin.* — Meridian work. Photographic work on star-cluster G.C. 1712. Latitude according to Talcott's method. Time service.

[To be continued.]

PUBLICATIONS.

THE CAPE TEN-YEAR CATALOGUE FOR 1890·0 *.—Dr. Gill has recently published his second star catalogue, resulting from the meridian observations made at the Cape Observatory from 1885 August 17 to 1895 December 31. The number of stars whose places are given in the Catalogue is 3007; the number of observations on which each place depends varies, but from a casual estimation it appears that the average is about 8. There are few stars which have been observed only once or twice, whilst there are many whose places are the result of more than 20 observations.

The columns of the Catalogue itself show some novel features. First, the R.A. and Dec. are given for Epoch 1890·0, but no correction for proper motion for the interval between this epoch and the epoch of observation has been applied. The annual P.M. is given, and also the correction suitable for application. In the page devoted to declination (each star has a line of a *double* page devoted to it) there are two principal columns; one, headed "Mean Dec.," contains the places from observation formed on the assumptions that the refractions of the *Tabulæ Regiomontanæ* are accurate, that the latitude is $-33^\circ\ 56'\ 3''\cdot28$, which is deduced from observations of circumpolar stars at upper and lower culminations; and a correction for flexure, $-0''\cdot30 \sin \zeta$, has also been applied, the constant having been determined from observations of opposing collimators, made generally once a week during the ten years. The second of these principal columns, headed "Sec. of Final Dec.," contains figures which result from applying corrections to the preceding column resulting from the assumptions that:—

(1) The latitude of the transit-circle is $-33^\circ\ 56'\ 3''\cdot45$;
(2) That the corrections for refraction computed from

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* 'A Catalogue of 3007 Stars for the Equinox 1890·0, from Observations made at the Royal Observatory, Cape of Good Hope, 1885–1895, under the direction of David Gill, C.B., LL.D., F.R.S., &c. With Appendices. London, 1898.

Bessel's *Tabulæ Regiomontanæ* should be multiplied by $(1-0{\cdot}00218)$;
and (3) corrections for the variation of latitude (computed by Dr. Albrecht).

This new value of the latitude results from the application of the corrections for flexure, refraction, and variation of latitude to the observations of circumpolar stars. The catalogue therefore gives the declinations as deduced from adoption of two distinct sets of data—a scheme which has much to recommend it, although it may lead to computers' mistakes in the use of the volume.

So far nothing has been said about observations of stars by reflexion. Nearly 1300 such observations have been made, but they have not been used in the formation of the catalogue. The quantity R−D is tabulated in the introduction; first uncorrected, and then corrected for inclination of the vertical and for flexure; of which it may be said that the corrections do not by any means reduce the amount of the tabulated quantities.

The Right Ascensions of the catalogue depend on the places of Dr. Auwer's *Fundamental Catalog die Zonen Beobachtungen am nördlichen Himmel.*

An interesting feature of the Introduction to the catalogue is the comparison with five other catalogues—Cape 1880, Melbourne 1880, Greenwich 1880, Cape 1885, and Radcliffe 1890, the differences being arranged according to R.A. and according to Declination. There follows also a similar comparison with the differences tabulated according to magnitude of the star. In the differences of declination there is no sensible systematic difference depending on star-magnitude, and even in the case of the Right Ascensions there is no well-marked discordance from the same cause; but Dr. Gill seems to have assured himself, from the evidence of heliometer and photographic measures compared with transit observations, that observers systematically observe faint stars too late at the rate of $0^{s}{\cdot}01$ per magnitude, and thinks that corrections should be applied to the places of the present work from this cause. Observations to investigate this point were made by the different observers. From these it appears that when a screen which reduced the magnitude 1·8 in brightness was interposed, the time of transit was made larger by a quantity $0^{s}{\cdot}025$, this being the final mean for five observers, observing stars of all magnitudes chronographically. No correction from this cause has been applied; but Dr. Gill gives a table of corrections ($+0^{s}{\cdot}056$ for a star of mag. 0·0, $-0^{s}{\cdot}070$ for a star of mag. 9·0) applicable to the catalogue places, taking the fourth magnitude as standard, and proposes to apply such corrections in future. In our opinion the evidence scarcely warrants such application. In the first place, the interposition of a screen changes the form and character of the image rather than the magnitude; and, secondly, it is not impossible that the screen actually optically alters the direction of the line of sight—possibly, but we have no information how the screen was disposed: attaching it to the telescope might mechanically alter the line of collimation of the telescope; but this is outside the limit of this notice, for no correction has been applied in the present work, and there is nothing left for the reviewer but to congratulate Dr. Gill and his staff (Mr. J. Power is specially mentioned as having contributed largely and carefully to the construction of the catalogue) on the prompt completion of a valuable work which does credit to the Cape Observatory.

H. P. H.

STELLAR PHOTOMETRY *.—The recent appearance of vol. xxiii. part ii. of the 'Harvard Annals' is somewhat opportune, dealing as it does with star-magnitudes, a subject likely to be much under discussion now that the publication of the Astrographic Catalogue is coming into view. The volume is entitled "Discussion of Observations made with the Meridian Photometer during the Years 1882-88," by E. C. Pickering and O. C. Wendell, and is made up of chapters on the magnitudes of the Uranometria Argentina, of the Bonn Durchmusterung, on those determined by Sir William Herschel, and on the Harvard Photometry.

The work consists mainly of tables of comparison, prepared and explained in the careful way with which those who know the Harvard volumes are familiar. It would take too much space to consider all of these in detail; but the facts about the Durchmusterung have special claims on our attention.

The Meridian photometer with which the observations were made (the actual observations were published in vol. xxiv. of the 'Annals') consists of a pair of similar telescopes placed horizontally and side by side. By mechanical arrangement of prisms it is possible to make the image of some close polar star, used as standard, appear in the field of one telescope, and the image of another star which is passing the meridian in the field of the other. Both images are viewed at once by the same eyepiece; but before coming to focus the pencils of light pass through an optical arrangement, so that by rotating a Nicol prism the images viewed can be equalized, and by a reading giving the position of the prism the relative light of the two stars is determined. The polar star used as standard for these observations was λ Ursæ Minoris, its magnitude being continually compared with that of other circumpolar stars to check any possible variation in its brightness. By this process it is possible to determine the accurate photometric relation of stars; the next step is to compare these magnitudes with those determined by other methods, to ascertain whether the standard, which is quite arbitrary, or at

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* 'Harvard Observatory Annals,' vol. xxiii. part ii. Cambridge, U.S.A.: 1899.

best depends on the adopted magnitude of the comparison-star (λ Ursæ Minoris, in the present instance), agrees with the standards usually adopted.

Prof. Pickering measured nearly 17,000 stars contained in the Bonn Durchmusterung for determination of the relation of the magnitudes of the two systems, and then 3000 more. These two series of observations are discussed separately but similarly. The residual—Photometric magnitude minus B.D. magnitude—was formed for each star. The stars were divided into groups according to magnitude, the first containing stars brighter than 3·8, the next stars of magnitudes 3·8 to 4·2, and so on, each group embracing half a magnitude; these are divided into subgroups, according to position in the sky, and the mean residual for each is tabulated. Cursory inspection of the table shows that for stars fainter than 7·0 the residual is positive, and has an average value of about three-tenths of a magnitude—that is, the photometric magnitudes are fainter by this quantity; for stars brighter than 7·0 no general statement can be made. Especially it may be noticed that the residuals are smallest, that is the B.D. magnitudes are faintest compared with the photometric, in the regions which contain the Milky Way. A second arrangement of the residuals, the division of the stars of each magnitude being made according to declination, shows a distinct variation. Stars culminating near the zenith have the largest residuals—that is, for stars far from the zenith the magnitudes determined by Argelander are too faint, the result of atmospheric absorption, the photometric measures having been corrected for the effect of this.

So far the discussion consists of comparison of the Harvard magnitudes with those of Argelander, and such comparison has been made for the two series of observations of 17,000 and 3000 stars above mentioned; but there is an entirely different method of fixing standards of magnitude known as the "method of equable distribution." The following illustrative case may explain the method:—If of a certain number of stars one person says there are 4 of the first magnitude, and another says there are only 3, it is clear that the latter person uses a brighter standard as the division between first and second magnitude stars, and these numbers 4 and 3 can be used to compare the standards of the two observers. In other words, equivalent magnitudes in two scales are defined by the fact that if the stars in the sky are photometrically measured, according to two scales, the number of stars in both systems brighter than magnitudes which are equivalents should be the same. In this way the magnitudes of the Durchmusterung and of the meridian photometer have been compared. Prof. Pickering thus has five distinct comparisons of magnitudes, and the discussions of the resulting differences lead to some points of interest. First, it appears that in regions near Orion and Canis Major the stars in the Duchmusterung are estimated too faint, which is ascribed to the proximity of the bright stars. Secondly, that the observations with the meridian photometer show no effect of atmospheric absorption dependent on the season of the year, and that the corrections which have been applied for this error are trustworthy. Thirdly, that the D.M. magnitude of stars in the neighbourhood of the Milky Way are faint. Those between the equator and +20° are bright owing to their greater altitude when they were observed at Bonn. Finally a table is given, which we take leave to reproduce, giving final comparison of stars of different magnitudes in the Durchmusterung and the Harvard system :—

Equivalent Durchmusterung and Photometric Magnitudes.

D.M.	Phot.	D.M.	Phot.	D.M.	Phot.	D.M.	Phot.	D.M.	Phot.
1·0	0·3	2·7	2·9	4·4	4·4	6·1	6·0	7·8	7·8
1·1	1·0	2·8	3·0	4·5	4·5	6·2	6·1	7·9	7·9
1·2	1·1	2·9	3·1	4·6	4·6	6·3	6·2	8·0	8·0
1·3	1·3	3·0	3·2	4·7	4·7	6·4	6·3	8·1	8·1
1·4	1·4	3·1	3·3	4·8	4·8	6·5	6·4	8·2	8·2
1·5	1·5	3·2	3·4	4·9	4·9	6·6	6·5	8·3	8·4
1·6	1·6	3·3	3·5	5·0	5·0	6·7	6·6	8·4	8·5
1·7	1·7	3·4	3·6	5·1	5·1	6·8	6·8	8·5	8·6
1·8	1·9	3·5	3·7	5·2	5·2	6·9	6·9	8·6	8·7
1·9	2·0	3·6	3·8	5·3	5·2	7·0	7·0	8·7	8·8
2·0	2·1	3·7	3·9	5·4	5·3	7·1	7·1	8·8	9·0
2·1	2·2	3·8	3·9	5·5	5·4	7·2	7·2	8·9	9·1
2·2	2·3	3·9	4·0	5·6	5·5	7·3	7·3	9·0	9·3
2·3	2·5	4·0	4·1	5·7	5·6	7·4	7·4	9·1	9·5
2·4	2·6	4·1	4·2	5·8	5·7	7·5	7·5	9·2	9·7
2·5	2·7	4·2	4·3	5·9	5·8	7·6	7·6		
2·6	2·8	4·3	4·3	6·0	5·9	7·7	7·7		

The other sections of the volume are researches on similar lines. The first investigation undertaken with the Meridian Photometer was the determination of the brightness of the stars adopted as standards of magnitude in forming the Uranometria Argentina. These are 722 in number, of magnitudes included between 2·7 and 7·9, and their declinations are mainly between +5° and +15°. These observations are discussed by Prof. Pickering with a main average result that it is found that the photometric measures show stars to be brighter than they might be supposed to be from the magnitudes in the U.A. The residuals are also grouped according to position in the sky, from which it appears that the stars in the region of the Milky Way are estimated relatively too faint in the Argentine work. This grouping is valuable, remembering the remark in Dr. Gill's introduction to the Cape Photographic Durchmusterung; speaking of the Argentine magnitudes he says: "besides the systematic differences between visual and photographic magnitudes depending on galactic latitude, it is not impossible that there are special regions of the sky where local differences occur." Inspection of the table shows that, apart from the Milky Way regions, the differences depend more on the magnitude of the stars than on position in the sky.

The section devoted to the observations of Sir William Herschel gives a comparison of his magnitudes of the stars in Flamsteed's British Catalogue with the magnitudes of the same stars as determined by the photometer. This comparison is printed according to constellations, and copious notes are added. It is impossible to give any summary of this, and it is equally impossible within our small limits of space to do justice to this valuable contribution to the subject which is such a distinctive feature of the work of the Harvard Observatory.

THE STORY OF ECLIPSES *.—This book deserves very harsh treatment at the hands of the reviewer. It is one of a series of which we believe Mr. Chambers has written three others, and is written "because public attention is already being directed to the forthcoming total eclipse of the Sun on May 28, 1900." It is well to note this reason, because, with that in mind, the book itself seems to be such a charming example of inconsequence. The author begins by being superior at the expense of "the man in the street," who takes no interest in science, and then proceeds to give him about 150 pages out of 253 of ancient history relating to eclipses, which is rather less interesting than a dictionary of dates. Why did he not tell him (the man in the street) what are the problems that astronomers are trying to solve when they go abroad to observe eclipses? Mr. Chambers has not done this in anything approaching an adequate manner, but sums up the corona problem in a few paragraphs beginning "The most recent important eclipses up to date which have been observed, namely those of April 16, 1893, Aug. 9, 1896, and Jan. 21, 1898, do not much add to our useful records of the outward appearances presented by the corona." This seems to express Mr. Chambers's attitude towards modern astronomy. We believe he left off taking an active part in the science some years ago, and he appears to think that the growth of astronomy stopped then. He certainly pays the well-deserved compliment to the Astronomer Royal's coronal photographs of 1898, but the spectroscopic work of the last three eclipses is condensed into a casual remark that "one important result obtained at Nova Zembla was a full confirmation by Mr. Shackleton of Prof. Young's discovery in 1870 of the 'Reversing Layer,' a discovery which was long and vehemently disputed by Sir Norman Lockyer." He evidently thinks that the "general public" for whom he is writing knows exactly what a Reversing Layer is.

We have said that Mr. Chambers has devoted much space to

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* By G. F. Chambers, F.R.A.S. The Library of Useful Stories. London: George Newnes, Ltd. 1*s*.

the history of eclipses. He has not made one reference to the late Mr. Cowper Ranyard's monumental contribution to the subject, but he has referred largely to the Rev. S. J. Johnson's book. It may be added in passing that he rather takes the edge off this compliment by spelling Mr. Johnson's name incorrectly eight times out of seventeen. He mentions Mr. Lynn's name in the following manner: "both these theories have been called in question by Johnston (*sic*), because he cannot find an eclipse which, in his view of things, will respond as regards date . . . . and by Lynn on the same and on other grounds *more suo*." What the words *more suo* imply is not obvious. If it is a sneer it is a rather badly advised one, for all astronomers know that there is no one who knows more about the history of eclipses, or takes more care in his references, than Mr. Lynn.

Mr. Chambers's emendation of Shakespeare, "*shivered* in the Moon's eclipse," for *silvered*, is hardly an improvement; also in the legend of Ho and Hi, the second line of the verse is generally written:

> "Whose fate though sad was risible."

Mr. Chambers prefers to write "visible." But these are small points, and may be merely typographical. Our quarrel with the book is for a larger reason. Mr. Chambers evidently has a reputation as a book-compiler, or publishing firms would not ask him to contribute to a series of works such as this; it is no concern of ours whether he loses or retains this reputation, but we are concerned to think that the world, or such part of it as reads this book, should be told, as they are told by it, that the chief part of astronomy is its ancient history, and that nothing is being done in the present day to advance the science.

NOTES.

Comet Notes.—The discovery of a faint comet by M. Giacobini on Sept. 29 was announced last month, but owing to haste the motion was erroneously given. The following are the best elements available, the first by Herr Winther from observations on Oct. 3, 6, 9 (Ast. Nach. 3598), the second by Mr. Perrine from observations on Oct. 1, 7, 16:—

	Winther.	Perrine.	
T	1899 Sept. 18·27.	1899 Sept. 15·04 Greenwich M.T.	
ω	12° 47′	10° 52′	1899·0.
☊	272 5	272 13	
i	76 33	76 55	
log *q*..	0·25273	0·25174	

Ephemeris for Berlin midnight.

	R.A.	N. Dec.		R.A.	N. Dec.
	h m s	° ′		h m s	° ′
Nov. 2....	17 18 59	4 42	Nov. 14....	17 38 10	7 57
6....	17 25 18	5 47	18....	17 44 43	9 2
10....	17 31 42	6 52	22....	17 51 21	10 7

Brightness 0·63 on Nov. 5, 0·52 on Nov. 22.

We may notice a singular error in 'Nature' for Oct. 5, p. 551, where the three comets discovered by M. Giacobini (1896 Sept., 1898 June, 1899 Sept.) are spoken of as one and the same comet. They are, of course, entirely distinct and separate.

Herr Zwiers sends a post-card stating that he has received altogether nine observations of Holmes' Comet from Mr. Perrine, and five from Mr. Aitken. The observations were all made with the 36-inch at the Lick Observatory, and extend from June 10 to Sept. 9. They indicate that the adopted value of the mean anomaly in Herr Zwiers' corrected elements requires correction by 8″ (he does not say in which direction). The comet is very faint and only visible in the most powerful instruments.

Continuation of the ephemeris (for Greenwich noon)
(Ast. Nach. 3595).

	R.A.	N. Dec.		R.A.	N. Dec.
	h m s	° ′		h m s	° ′
Nov. 2....	2 36 37	49 14	Dec. 8....	2 5 30	45 35
8....	2 29 21	49 2	14....	2 4 28	44 42
14....	2 22 32	48 37	20....	2 4 43	43 50
20....	2 16 32	48 1	26....	2 6 10	43 0
26....	2 11 36	47 17	Jan. 1....	2 8 43	42 14
Dec. 2....	2 7 53	46 28			

The brightness reached a maximum about Oct. 15, when it was 1·8 times that at re-discovery. On Dec. 26 it will have sunk again to that at re-discovery.

Tempel's Second Periodic Comet was observed by Signor Cerulli at Teramo on Oct. 2, the corrections required by Schulhof's ephemeris being $-1^{s}{\cdot}9$ in R.A., $+3''$ in Decl. It had a nucleus of the 14th magnitude.

Herr Adolf Hnatek, of Vienna, proposes to compute the definitive orbit of Comet 1898 V. (Giacobini), and requests that unpublished observations may be sent to him (Ast. Nach. 3597).

A. C. D. C.

Minor Planet Notes.—Ast. Nach. 3597 contains a new determination of the orbit of (433) Eros, by Hans Osten. About

220 observations are employed, extending from 1898 Aug. 14 to 1899 March 17, but no use is made of the photographs of 1894 and 1896.

The following are the elements:—

Epoch and osculation 1898 Oct. 1·0 Berlin M.T.

M	238° 39′ 44″·6	±15″·8	1900·0.
ω	177 39 21·0	±15·2	
☊	303 31 53·4	± 2·6	
i	10 49 34·0	± 0·5	
φ	12 52 18·3	± 2·6	
μ	2015″·343	±0″·073	

A planet, EQ, discovered last month, proves not to be new, but identical with (161) Athor.

Three new planets, ER, ES, ET, were discovered on Oct. 27 by Wolf and Schwassmann. A. C. D. C.

OBITUARY.—JAMES CARPENTER. By the present generation of astronomers Mr. Carpenter's name will probably only be known as that of the joint author with Sir James Nasmyth of a celebrated book on the Moon, but some thirty years ago he was an active member of the Greenwich staff. Mr. Carpenter belonged to a Greenwich family and became an Assistant at the Royal Observatory in January 1859. He worked in various departments of the Observatory, and when the south-east equatorial was set up in 1868 he was put in charge of that instrument, and there are preserved in the Observatory records many excellent planetary drawings made by him from observations with the 12½-inch Merz. He married a daughter of Thomas Penn, cousin of John Penn the elder, and in 1872 gave up his Assistantship at the Observatory to take up a position in the famous engineering firm which his father-in-law had long held. Since that time, except for an occasional attendance at Burlington House, he has taken little part in astronomy, and he gave up work entirely some years ago. He died almost suddenly, as the result of heart-complaint, from which he had long suffered, on October 17, aged 59. Mr. Carpenter's wife pre-deceased him, and he leaves no family.

THE PARALLAX OF Σ 1516 A AND AOE 11677.—Herr Osten Bergstrand in 1897 and 1898 took a number of photographs of the regions around these two stars with the idea of determining their parallax. The instrument was the 33-cm. photographic refractor of the Upsala Observatory. Previous researches on the parallax of Σ 1516 have been made by Winnecke, Berberich, and L. de Ball, who all used one star only for comparison, viz., Σ 1516 B, and deduced values ranging from 0″·1 to 0″·3. The present determination depends upon four comparison stars, which give values as follows:—

B.D. +73°·518	gives parallax	+0″·06
B.D. +74 ·455	„	+0 ·19
Σ 1519 B	„	+0 ·04
B.D. +74°·457	„	+0 ·09

The definitive value for the parallax of Σ 1516 A from the four is

$$\pi = +0''\cdot080 \pm 0''\cdot011,$$

which agrees more closely with that of M. de Ball, viz., 0″·104.

The star AOe is remarkable by reason of its large proper motion (nearly 3″ per ann.). M. Geelmuyden has found a parallax of 0″·4, employing but one comparison star. M. Franz using two stars found 0″·1002. The present determination is based on eight stars. The separate values from individual stars are from

B.D. +66°·711	parallax	+0″·22
B.D. +66 ·712	„	+0 ·29
B.D. +66 ·714	„	+0 ·22
B.B. +66 ·715	„	+0 ·12
B.D. +66 ·716	„	+0 ·10
B.D. +67 ·694	„	+0 ·10
B.D. +67 ·694	„	+0 ·10
Anonymous	„	+0 ·18

which give a definitive value

$$\pi = +0''\cdot192 \pm 0''\cdot013.$$

THE FIGURE OF MARS.—In Ast. Nach. No. 3594 Dr. Ernst Hartwig gives his observations of diameters of Mars made in 1879 with the heliometers at Bamberg and Breslau. These observations he discusses at length and deduces for

Equatorial diameter	9″·445
Polar diameter	9 ·364

which gives $\frac{1}{116}$ for the compression.

Similar observations made at the oppositions 1890 and 1899 give for

1890 .... Equatorial diameter 9″·435, polar 9″·303
1899 .... „ „ 9 ·370, „ 0 ·270

and values $\frac{1}{71}$ and $\frac{1}{94}$ respectively for compression.

A DETERMINATION OF THE CONSTANTS OF ABERRATION AND NUTATION.—In German Universities it is generally required of candidates for the degree of Doctor that they make an original research on some scientific subject, and this is naturally sometimes on an astronomical subject. We have recently received such a one from Herr Johannes Kniesche, of the University of Gottingen. This is a discussion of the Greenwich observations of the Right Ascension of Polaris from 1851 to 1888 to determine the constants of aberration and nutation. We need say nothing of the method, except that account was taken of the Variation of Latitude, and that it evidently involved an immense amount of arithmetic. The results evolved are:—

Aberration constant	$=20''\cdot537$	$\pm0''\cdot003$
Nutation constant	$9\cdot236$	$\pm\ 0\cdot003$
Parallax of Polaris	$+\ 0''\cdot1518$	$\pm0''\cdot0041$

RELATIVE STAR-DENSITY ON PHOTOGRAPHS AND BY VISUAL OBSERVATION.—In a notice in our July number of a volume of the Potsdam Astrographic Catalogue, mention was made (p. 280, line 16) of the relative number of stars in the same area as counted on the Potsdam photographs and as given in the Bonn Durchmusterung. Prof. Scheiner had called attention to this some months previously in an article in Ast. Nach. No. 3505, and this gave rise to remarks on the same subject by Dr. Kapteyn and Prof. Newcomb. Prof. Scheiner, in speaking of relative density on equal areas, wrote: "In one case the ratio (*d*) of the star-density between Potsdam and the D.M. is unity. In another, in the Milky Way, it reaches 11·7. But for the whole series of plates the facts can be represented graphically as follows:—If abscissæ represent the number of stars on a plate and the ordinates are taken proportional to *d*, the resulting locus is very nearly a straight line, the conclusion from which is that the density ratio between Potsdam and the D.M. is proportional to the density itself."

An interpretation of this last statement would be that stars are equally distributed throughout any direction in space, which would give roughly such a law for star-density as Scheiner's, assuming that the photographic plate comprehends a sphere of the universe, the ratio of whose radius to that of the sphere to which visual observations extend is constant in all directions; but it may be well to consider whether the law is borne out by other investigations. In the Cape Photographic Durchmusterung, for reasons which Dr. Gill points out, no detailed accounts of this kind are given, but it is stated that this catalogue is richer in the rich regions and poorer in the poor regions of the sky than Schönfeld's continuation of the B.D., which is a somewhat similar statement to that quoted above. Prof. Newcomb, however, seeing the interest of the subject, made a count of the stars in the Cincinnati catalogue of Ormond Stone, of the zone —23°. Comparing the number of stars in this with those of the Cape Durchmusterung, he found that the greater the number of stars in any region the greater the excess of their number in the Cape work over that in the work of Stone. In the denser regions Kapteyn's work shows a little more than twice as many stars as Stone's, whereas in the whole work the number is greater by not quite fifty per cent. So far Dr. Scheiner's proposition is supported, but the figures deduced from Greenwich catalogue plates given in our July number show only a faint trace of such a law.

There is another question which has been treated of side by side with this of star-density; that is, the magnitude of stars in the region of the Milky Way relative to that of stars in the other parts of the sky as determined by visual and photographic methods. In the recently published volume of the Harvard Annals* it is shown that, both in the B.D. and in the Uranometria Argentina, stars near the Milky Way are faint as compared with accurate photometric determinations. Dr. Scheiner evolves a similar fact from the Potsdam photographs; he finds that of the stars about magnitude 9·0 to 9·5, those in the Milky Way are about half a magnitude fainter than they would be in a poorer region. Dr. Kapteyn also agrees that the visual magnitudes of stars in the Galaxy are faint in comparison with their photographic magnitudes, and states as a possible reason that stars in the Milky Way are bluish. On the other hand, in the comparison above mentioned Prof. Newcomb could detect no difference in relative magnitude depending on the density of the stars.

A Star Position Finder.—In Sir H. Grubb's catalogue (see p. 378), on the page devoted to miscellaneous apparatus, we noticed an illustration of an accessory to an equatorial for reading the N.P.D. and hour-angle at which the telescope is set without leaving the eye-end. As this little instrument is probably not well known, and the geometry is ingenious, it may be worth while to attempt an explanation. Everyone is familiar with the method of setting a small transit by means of a bubble and small graduated circle, the circle being attached near the eye-end at the side of the telescope with its plane at right angles to the axis. Now on an equatorial put a similar circle with plane at right angles to the declination axis. In the usual case the rotating radius which carries the verniers for reading the circle is furnished with a level-bubble parallel to the line of verniers; in the present case the radius is furnished with a circular level with its plane making an angle with it equal to the latitude of the place. Now if the telescope be in any position and the level set horizontal, since the rotating radius is at right angles to the axis through the circle (*i. e.* the declination axis) and makes an angle with the horizon equal to the latitude, it must be parallel to the polar axis, and the angle that the telescope makes with this (*i. e.* the N.P.D.) is read off on the circle.

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* See p. 402.

Further, the circular level is mounted on the rotating radius, which may be considered as a polar axis of the contrivance, so that it has means of rotation about the radius as axis, and is furnished with a circle to read the angle through which it is turned. Now suppose the telescope set to the meridian, and the circular level adjusted, the circle reads 0. The equatorial being turned on its polar axis, the bubble is displaced, but can be brought back by rotating the circular level about the small polar axis of the position finder, and hence the hour-angle read on the circle.

The readings given are therefore those of the position of the telescope. Consideration will show that in only that position of the telescope could these readings result.

THE SUN'S ROTATION.—It is found difficult to give a satisfactory explanation of the difference in the time of rotation of the solar surface in different latitudes, but it is usually accepted as a fact in solar physics. Herr Schultz-Steinheil, of the Lund Observatory, pointed out some time ago (Ast. Nach. No. 3543) that the velocities as determined by Duner's spectroscopic observations did not show these anomalies if values of the solar elements different from Sporer's were adopted. He has followed this up by determining recently the values of these elements which would give a uniform period of rotation, and finds that the inclination of the Sun's axis to the axis of the ecliptic should be 18° 12′ instead of 7° 15′ as hitherto adopted, and that the longitude of the intersection of the Sun's equator with the ecliptic should be +28° instead of +75°.

There is not sufficient reason to think that Sporer's elements are beyond correction, but at first sight it seems that Herr Schultz-Steinheil's figures differ by more than is at all consistent with sun-spot observations.

THE Election for Council of the British Astronomical Association for the Session 1st October, 1899, to 30th September, 1900, has resulted as follows:—*President*: W. H. Maw. *Vice-Presidents*: N. E. Green, G. M. Seabroke, Dr. G. Johnstone Stoney, W. H. Wesley. *Treasurer*: W. H. Maw. *Editor*: E. Walter Maunder. *Secretaries*: James G. Petrie, William Schooling. *Librarian*: F. W. Levander. *Directors of the Observing Sections*: *Sun*, Rev. A. L. Cortie; *Moon*, Walter Goodacre; *Mercury and Venus*, Henry MacEwen; *Mars*, E. M. Antoniadi; *Jupiter*, Arthur Cottam; *Saturn*, G. M. Seabroke; *Comets*, A. C. D. Crommelin; *Meteors*, W. F. Denning; *Star Colours*, G. F. Chambers; *Variable Stars*, J. E. Gore; *Double Stars*, G. M. Seabroke; *Spectroscopy*, J. Evershed; *Photography*, R. Wilding; *Zodiacal Light*, Capt. P. B. Molesworth, R.E. *Other Members of the Council*: Rev. J. M. Bacon, A. M. W. Downing, Arthur Kennedy, G. J. Newbegin, Capt. W. Noble, Miss M. A. Orr, A. A. Rambaut, S. A. Saunder, David Smart, C. Thwaites.

MESSRS. COOKE AND SON'S NEW CATALOGUE.—This firm of instrument makers, which we notice has now a branch at Cape Town, as well as at York and in London, has anticipated the date by a month or two, and issued a Catalogue for the year 1900. Messrs. Cooke make leading features of two classes of work. First they have a monopoly of the achromatic objectives made according to Mr. Dennis Taylor's patents, which, it will be remembered, are made up of three lenses of three different sorts of glass, and so form an object-glass free from achromatism and from spherical aberration, which gives a large field with good definition, with little liability to the optical effects of flexure. A 3-inch object-glass of this kind costs £20, a 6-inch £90, and a 15-inch £800. Messrs. Cooke also make special lenses for photography. Other features of the catalogue are the pictures of the large domes put up by Messrs. Cooke; the two large domes at Greenwich, of 30 and 36 feet diameter respectively; the 36-foot dome at the Royal Observatory, Cape of Good Hope; and an 18-foot dome erected at Seapoint, Cape Town. The prices of "Standard" Domes, which are hemispherical with double-shutter giving an opening from the base to rather beyond the zenith, covered with *papier-mâché*, run as follows: 15-foot, £230; 30 foot, £590; 40-foot, £975.

An equatorial telescope of best quality, with a patent object-glass of 6-inch diameter fitted for both photographic and visual work, with driving-clock and usual accessories, including camera and plate-holders, costs at Messrs. Cooke's works £500. A similar instrument, but with object-glass of 15-inches aperture, costs £1950. The cost of a best-quality transit-instrument, with 3½-inch O.G., complete, would be about £100, but a smaller instrument can be bought for less than half that sum. The firm acknowledge that their prices are high, but they deprecate the modern tendency to sacrifice good workmanship to cheapness.

ERRATUM.—Mr. Lynn writes:—"Some of your readers may be surprised that I should speak, in my letter of August 18 (p. 366, line 6), of so large a year of Rome as 7045. But doubtless most of them will perceive that what I really wrote was 704–5, meaning that Cæsar began his march from Gaul towards the end of 704 (B.C. 50), and came to Rome early in 705 (B.C. 49)."

THE death is announced, at the age of 86, of the Rev. E. L. Berthon, the inventor of the collapsible boat. It will be remembered that he was admitted, for the second time, a Fellow of the Royal Astronomical Society at the meeting in June last, when he was paradoxically described as the Senior Fellow, although he had only just been admitted.

It has been announced in the daily press that the Lincolnshire County Committee has accepted the offer of a complete set of astronomical instruments, valued at nearly £1000, from the executors of the late Canon Cross, and propose to erect a suitable observatory at the north-west corner of the castle keep.

A Reuter's telegram from New York, published in the papers of October 30, states that Biela's Comet has been observed from Santiago, Chile, and is visible to the naked eye. A cautionary note is added to the telegram, saying that the statement must be accepted with reserve, as Biela's Comet has not been seen since 1852.

We call attention to the Occultation of Neptune by the Moon on November 19. The circumstances are given in the 'Companion.'

Prof. Simon Newcomb has been elected President of the recently established Astronomical and Astrophysical Society of America. Prof. G. C. Comstock is the Secretary.

A note in 'Nature' states that the highest observatory in Germany is now completed. It is situated on the Schnee Koppe, the highest summit of the Silesian Mountains, at an elevation of 5216 feet. It will be managed as an institution of the Prussian State.

M. Bouquet de la Grye, at a meeting of the Paris Academy, when a paper on meteor-observations was read, suggested that the observations of shooting-stars might be utilized for determination of longitude when the telegraph is not available.

The Meeting of the Royal Astronomical Society is on Friday, November 10; of the British Astronomical Association on Wednesday, November 29.

From an Oxford Note-Book.

At the Meeting of the British Association at Bristol last year, Prof. H. L. Callendar showed a beautiful apparatus for recording sunshine (see *Observatory*, vol. xxi. p. 391). Although the details had been then worked out with great completeness, several improvements have since been made by the Cambridge Scientific Instrument Company, who supply the machines; and as a preliminary to setting one (or perhaps two) up in Oxford, I had the pleasure of looking at the improved pattern at University College, London, the other day. It is quite delightful to watch the apparatus at work. There is a recording pencil which draws the curve of radiation, and a planimeter wheel which integrates the area automatically. When the contact is made which puts the instrument in action, little wheels buzz round, and the pencil runs along to its proper place with great promptitude in a charming way. The apparatus is one of the best of that class of instruments which add to scientific accuracy and refinement all the pleasures of a beautiful mechanical toy. When one adds that the instrument cleans itself as it goes, and so works all the better for use, it will be fully realized that it is a desirable possession.

AFTER examining the recording apparatus (which can be used not only to record sunshine, but temperature or any other elements which can be reduced to terms of electrical resistance) in the laboratory, we mounted to the roof of the building to see the situation of the plate exposed to sunshine. We stepped out of a window on to the slates, worked our way along copings, and clambered over roofs by the aid of ladders set at all sorts of angles, and so ultimately found ourselves commanding a nearly clear horizon from the top of a disused chimney communicating with the laboratory, down which the wires pass from the sunshine-plate to the recording apparatus. The chimney could scarcely have been more appropriate if it had been built for the purpose. But Prof. Callendar has the luck of a successful man in such matters. He showed me, on the way, a disused spiral stone staircase which he has obtained possession of, and intends to devote to a series of physical experiments, for which it might have been designed. What an opportunity for a cynic to compare the man who finds his laboratory or observatory ready-made in odd corners of an existing building and works therein, with the man who builds carefully-designed structures and forgets to do any work in them!

THE clambering over the roofs of University College reminded me of an amusing little book lately introduced to my notice by an eminent astronomer, and called the *Roof Climber's Guide to Trinity* *. It has apparently been the practice of certain daring spirits for many generations of undergraduates to roam over the roofs of the many Courts of Trinity College, Cambridge; not with any intention of breaking bounds or rules ("Unconventional roads in and out of College are purposely left unmentioned," says the preface; "they are an all too popular abuse of a noble science"), but simply from the desire to scale places difficult of scaling. The 'Guide' tells of achievements to the present date in a way calculated to fire the ambition of even the quite casual reader, and it cannot fail to amuse him, as perhaps a couple of extracts may illustrate:—

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* **Cambridge: W. P. Spalding, 43 Sidney Street. Price 1s.**

> NEW COURT.—Complete circuit; time 45–60 mins.; guide superfluous, but rope necessary; expense, repair of pipes and possible doctor's fees (p. 3).

> (The I Court climbs) were originally undertaken with a view to finding some ascent from ground to roof without the use of the customary window; the only other two known being unpopular—the Library traverse chimney ("the Library") on account of its severity, and the Chapel ascent by reason of its early locked gate ("the Chapel").

A PAPER by Dr. C. A. Schultz-Steinheil on the "Elements of the Sun's Rotation" * seems worthy of attention. Dunér made a series of spectroscopic observations on the limb of the Sun for velocity in the line of sight, and found that the time of rotation was different in different heliographic latitudes, taking the Sun's axis as determined from sun-spot observations, viz.,

$$i = 7^{\circ}, \qquad ☊ = +74^{\circ}.$$

Dr. Schultz-Steinheil re-determines the position of the axis from the spectroscopic observations themselves, and finds that a position

$$i = 18^{\circ}, \qquad ☊ = +28^{\circ}$$

will remove the apparent anomalies of rotation. It is scarcely credible that an adopted position of the Sun's axis which gives such good results for sun-spot observations (as a glance at any of the recent Greenwich volumes, for instance, will show) can be so much in error as 11°; nor yet does it seem likely that while the general surface of the Sun is rotating like a solid body about one axis, the spots should appear to be rotating about a different axis. But the paper cited seems to require either explaining or explaining away.

THE following poem from a weekly paper seems to show that some misconception still exists, in spite of numerous popular lectures, as to the probable fate of our planet, and as to the nature of shooting stars:—

To-morrow, or ——?

It came with a horrible rumbling roar
  In the deathly still of the night;
A crash, and all was chaos;
And the world, its little allotted course
  In the mighty universe run,
A sizzling, seething ball of flame,
  Dashed downward toward the Sun.
And away on another planet
  In the firmament, gleaming afar,
A little child cried: "Oh, mamma, look!
  See the pretty shooting star!"

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* Öfversigt af K. Sv. Vet.-Akad. Förhandl. 1899, No. 2.

THE OBSERVATORY,

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1899 November 10.

Prof. G. H. Darwin, LL.D., F.R.S., *President*, in the Chair.

Secretaries: H. F. Newall, M.A., and F. W. Dyson, M.A.

The Minutes of the previous Meeting were read and confirmed.

The President. I would remark that I have a sad duty to perform by announcing to you the death of the Rev. Mr. Berthon, who was admitted only at the last Meeting. He was a very remarkable man, better known, perhaps, with regard to his invention of collapsible boats than in the astronomical world, but he had taken great interest in astronomy for many years.

Mr. Dyson. 211 presents have been received since the last Meeting. Attention may be called to two volumes received from two great observatories—the Cape Catalogue of 3007 star-places for the epoch 1890·0, deduced from observations at the Cape of Good Hope Observatory in the years 1885 to 1895; and vol. xxiii. part 2 of the 'Harvard Observatory Annals,' which is a discussion of observations made with the meridian photometer,—also to a volume of the American Ephemeris Papers, which consists of a catalogue of Fundamental Stars for epochs 1875 and 1900, prepared by Prof. Newcomb. We have also received from the British Nautical Almanac Office, Precession Tables adapted to Newcomb's value of the precessional constant; from the British Astronomical Association, the Report of the Indian Eclipse, 1898; and from Mr. Lynn, a copy of Mr. D. P. Todd's work, 'Stars and Telescopes.' Prof. Weinek has sent us another part of his 'Photographic Atlas of the Moon,' and we have received from the Lick Observatory some beautiful photographs of nebulæ taken with the Crossley reflector, and photographs taken during the Solar Eclipse of 1898.

The President. These photographs received from Lick are very remarkable. Copies of some of them will be thrown on the screen during the evening.

A vote of thanks was accorded to the donors of the presents.

Mr. F. A. Bellamy (presenting a paper on the "Distribution of Stars Photographed at Oxford for the Astrographic Catalogue"). In the July number of the 'Observatory' there was published a table giving the distribution of stars in the Greenwich zones, +64° to +70°, and it occurred to me that it might be interesting, as we have about the same number of plates, more than 550, to prepare a similar table for the plates in the Oxford zone, +25° to +29°. The method of measurement adopted at Greenwich differs from that adopted at Oxford. At Greenwich a portion only of each plate is measured, so as to avoid overlap of adjacent plates; at Oxford the whole plate is measured; but this difference does not invalidate the comparison. The number of stars per square degree was found for each hour of R.A., and the results were compared with those for Greenwich. As there seemed to be considerable differences between them, similar results were found for Paris and Potsdam; all these are shown in diagram 1. It is interesting to notice the sudden rise and fall in the curves for Oxford, Paris, and Potsdam as the Milky Way is crossed, but the curve for Greenwich does not accord with the others. The difference is partly accounted for by the proximity of the Greenwich zones to the Milky Way—in other words, by the smaller galactic latitude. But galactic latitude alone is not enough to bring out all the features of the plates. I give in the paper a discussion from Argelander's 'Bonn Durchmusterung' (*i. e.* stars to 9·5 mag.) in both galactic latitude and longitude, which shows clearly that the longitude is important as well as the latitude. We cannot give such a discussion from the photographs, as so few are taken; this must remain until the greater part of the sky has been photographed and the plates measured.

Mr. Dyson then read a communication by himself and *Mr. Hollis*, "On a Comparison of Diameters of the Images of Stars on the Greenwich Astrographic Plates with the Magnitudes of the Bonn Durchmusterung." He said that the paper consisted mainly of tabular matter, and described it as work which would be of use in the determination of the magnitudes of the stars on the photographic plates. The object of the paper was not to obtain a law for the determination of magnitude from diameter, but to show the variations from plate to plate. The images on 232 plates had been compared with the magnitudes given in the 'Durchmusterung.' Each plate contained on the average 30 or 40 stars given in the 'Durchmusterung,' nearly all between the B.D. mags. 8·0 and 9·5. Through the somewhat limited range, 8·0 to 9·2, the diameters increased proportionally with the magnitudes, but there was not sufficient range so that a law could be founded. By a somewhat empirical interpolation a correction to the nominal B.D. 9·5 mag. had been deduced. Further comparisons had been made to see whether any difference existed in the B.D. magnitudes depending on the density of the stars in the sky, or any difference in the photographic and visual magnitudes depending on the part of the sky. The differences due to the three kinds of plates used at the Royal Observatory—Ilford Special Rapid, Mawson and Swan, and Rocket Plates—have also been exhibited.

Mr. Hollis. This paper is not intended as a contribution to accurate photographic science. I do not think that the photographic plate at present can be used as a photometer, but in the course of every-day work at Greenwich we have used it as such in a rough way. If, for instance, we see a star whose diameter is about a tenth of a millimetre, we say at once that the star is of about the 9th magnitude. If it is about three times as large we know that we are looking at about a 6th-magnitude star. Now this paper is just an amplification of that. It gives a rule by means of which anyone can tell, from its measured diameter, the magnitude of the star. In the course of the work a circumstance has shown itself which may be an illustration of a fact already pointed out by various persons, which may be thus described:—We find on the astrographic plates that we have got practically all the Argelander stars of mag. 9·5, and these are represented, we may say, by diameter 25. But we have also on the plates many other stars of diameter 25 which are not in the B.D., and this may be because some stars just on the limit appear fainter than 9·5 visually because they are *blue*, and so do not occur in the B.D., but they appear brighter than 9·5 on the photographs.

Prof. H. H. Turner then read a paper "On Variation of Personal Equation with Stellar Magnitude in Meridian Observations at Cambridge, Berlin, and Greenwich, as deduced from Measures of Photographs made at Oxford." This paper, in common with the others, is a sample of the sort of results we shall get—and, in fact, are getting already—from the great international scheme for charting the heavens. The most immediate result is that relating to the number of stars in different parts of the sky, as in Mr. Bellamy's paper. Another is the relation between visual and photographic magnitudes. Dr. Kapteyn has already made an important generalization in this connexion, that the relation changes with galactic latitude; and though this conclusion has recently been questioned by Dr. Scheiner, I think Kapteyn's position is unshaken. My own paper deals with a result of a different kind altogether. It has been recognized in recent years that meridian observations of stars are affected by a systematic error depending on the magnitude of the star. Dr. Auwers, I believe, was the first to call attention to it. He found that he could not reconcile transit observations with heliometer observations unless he adopted some hypothesis of a magnitude equation; and he roughly determined the amount of this equation. Dr. Gill and others took up the matter, and found that this magnitude equation existed, and suggested as a means of measuring it that the observer should observe transits of stars with and without a gauze screen in front of the object-glass, producing a known diminution of the light. By comparing the transits observed in those two ways it was suggested that this magnitude equation might be determined. When we began to measure the plates for the Astrographic Catalogue, it occurred to me that photography afforded an independent check upon this question, because the photographs are not affected by this idiosyncrasy. If there is any tendency to place the cross wires of a measuring apparatus to the right or left of the star, it can be detected and eliminated by turning the photograph round through 180° in its own plane and remeasuring it. Mr. Hinks, of Cambridge, read a paper on the subject a few years ago, and gave the results of a few plates. At that time we had only 10 plates available for discussion, but now we have over 600. The magnitude equation comes out with the greatest clearness and accuracy. [Screen.] From mag. 5 to mag. 8 there is a drop of nearly a tenth of a second of time, the change being accurately $0^s{\cdot}023$ per magnitude.

This result is directly applicable only to the meridian observations at Cambridge; but a comparison of the Cambridge Catalogue with the Greenwich Ten-Year (1880·0) Catalogue shows that there is good agreement between them for all magnitudes from 1 to 8; and it seems possible that (as Dr. Gill indicates in the Introduction to his 1890 Catalogue, which I only saw an hour or two ago for the first time) this magnitude equation may be fairly constant for all observers. Dr. Gill finds a different value for the coefficient, $0^s{\cdot}014$ per magnitude instead of $0^s{\cdot}023$ as above. I attribute the discordance to a fault in the method of screens. I do not believe in the accuracy of the photometric value attributed to the screen, and I shall be interested to hear how the measurements on the Cape photographs accord with the results derived from screens.

For stars fainter than magnitude 8·0, the change is much greater at Cambridge (the comparison with Greenwich does not go beyond mag. 8·0), about $0^s{\cdot}100$ per magnitude. The line bends very sharply, as is shown in the diagram on the screen, forming a sort of hyperbola. The declinations of stars show no magnitude equation until we reach very faint stars.

Mr. Dyson. I should like to ask one or two questions about Mr. Bellamy's paper. The number of stars per square degree is a thing so easily determined that it is naturally one of the earliest, as it is one of the most interesting results one can hope to get from the astrographic work. From actual counting of the stars in the Bonn Durchmusterung, it has been found that the ratio of the number of stars of any magnitude to that of the magnitude fainter is 3 per magnitude. That is to say, there are three times as many stars between the 8th and 9th magnitude as there are between the 7th and 8th. This holds down to magnitude 9·0. It will be interesting to see whether the number of fainter stars seen on the photographic plate will follow this law. The two questions I should like to ask Mr. Bellamy are:—Is the ratio of the number of stars photographed per square degree to the number observed visually down to magnitude 9·0 constant for the dense and the sparse regions of the sky? and, secondly, Is the ratio of the number of stars shown with a 3^m exposure to the number shown with 1^m and with 20^s constant? Speaking quite roughly from impression, and not from actual counting, I should say that the ratio of the numbers of stars shown with exposures of 20^s, 3^m, 6^m, 40^m are pretty constant on the Greenwich photographs, but, as Mr. Bellamy pointed out, the part of the sky which they have photographed at Oxford is much more varied in the number of stars per square degree.

Mr. Bellamy. I noticed that point. The number of stars photographed with 20^s exposure at Oxford is about 0·8 of the number in Argelander on the average, and the number with 3^m is about 3·8 times Argelander. I may say that I also compared some plates in the Milky Way with Argelander's in a similar manner. Taking from our measures the mean diameter of stars given by Argelander as 9·5, I found that there were on the plates often 30 to 40 other stars with this diameter (or greater) *not* in Argelander; on one plate there was an excess of 120 stars.

Mr. Franklin-Adams. If much time is to be given to the collection of statistics of "magnitude equation" I would, with much diffidence, ask whether it would not be well to take note of the various forms of *illumination.* It may be found that the results differ. The two best-known methods are: first, the illumination of the field by a mirror diverting rays of light entering at one end of the axis; second, the illumination of the wires on a dark field. This has made some progress for use with faint stars, one of the best systems being the diffusion of light through a ring of glass round the wires, introduced, I believe, by Adam Hilger. But the system to which I beg particularly to refer is the lighting of the field by directing rays of electric light upon a prism cemented to the centre of the object-glass. The apparent illumination of stars of various magnitudes can be made so nearly alike by varying the quantity and the colour of the light used, that the magnitude equation from observations made under these conditions may be found to be very materially reduced or even entirely done away with.

Prof. Turner. I think it would be more satisfactory to divide the illumination of the field into *two* classes—sufficient and insufficient. I think when we have sufficient the magnitude equation seems to be constant for most observers. When the illumination becomes insufficient—that is, when we are rather straining our eyes to see faint stars,—then we get variations in the magnitude equation.

Mr. Dyson. That illumination has been tried at Greenwich, but I cannot say anything about the results. The worst of it is, it throws a shadow. If there are any scratches or defects in the eyepiece, shadows are cast, as the light comes from one point. That is the objection. The light can be regulated very easily and nicely.

The President. These three papers show to what interesting results the great photographic survey of the heavens will ultimately lead. We shall in time attain to fairly accurate ideas of the distribution of the stars in the universe; half a century ago such an idea would have seemed too ambitious even to dream of. The result obtained by Prof. Turner about the magnitude equation struck me as peculiarly interesting. I am sure the meeting will accord a hearty vote of thanks to the authors of these three papers.

Mr. Wesley showed on the screen and described photographs taken with the Crossley Reflector at the Lick Observatory of the Cluster in Hercules M 13, the Trifid Nebula M 20, the Spiral Nebula in Ursa Maj., the Spiral Nebula in Can. Ven., the Dumbell Nebula in Vulpecula, and the Ring Nebula in Lyra *.

The President. We owe our thanks to Prof. Keeler for sending us these wonderful pictures. They appeal especially to us because they were made with the speculum which was used by Dr. Common many years ago to obtain his well-known photograph of the Nebula in Orion. We also owe our thanks to Mr. Wesley for the admirable manner in which he has reproduced the photographs and for his demonstration of their salient points.

Mr. E. B. Knobel then took the Chair whilst *Prof. Darwin* read his paper "On the Theory of the Figure of the Earth carried to the Second Order of Small Quantities," the substance of which is as follows:—

The 'Philosophical Transactions' for 1826 contains a paper by Airy in which the formula for gravity is obtained inclusive of small quantities of the second order. Airy also established the conditions that the internal strata of the Earth may be level surfaces, but he left the two conditions in the form of integrals, because he found himself unable to solve the differential equations to which they are reducible. The present paper contains the required integrals.

In two memoirs in the Annals of the Paris Observatory for 1889, M. Callandreau has carried out an elaborate investigation of the same problem; but the author believes that he has given the results in a much more tractable form than that in which M. Callandreau left them.

The form of the internal surfaces of equal density and of the Earth's surface may be defined by the ellipticity and by a second parameter which defines the departure of the surface from the true ellipsoidal form. A considerable simplification in the work is, however, effected by replacing the ellipticity by another function which involves the ellipticity and the second parameter. The two conditions referred to above lead to two differential equations, of which the first involves only the new parameter. This corresponds with the differential equation for internal ellipticity given in treatises on the figure of the Earth, but it involves quantities of the second order as well as of the first.

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* See p. 437.

The second equation involves only quantities of the second order, and its solution is needed if the departure of the Earth's figure from a true ellipsoidal form is to be determined. The solution can only be effected when a definite hypothesis is made as to the distribution of density in the Earth. The author adopts Roche's hypothesis, and finds by the solution of the equation that the Earth's surface should lie below that of the true ellipsoid by 3¼ metres in lat. 45°.

Professor Wiechert * has recently suggested that the Earth consists of an iron nucleus with a rocky superstratum. This hypothesis leads to the conclusion that the departure from the ellipsoid amounts to 2¾ metres. Since two hypotheses so different as those of Roche and Wiechert lead to results so nearly concordant, it may be assumed that the true departure is about 3 metres, and is, in fact, so small as to be negligible.

It appears from M. Radau's remarkable investigations that the differential equation of the ellipticity is reducible from the second to the first order, at least to a close degree of approximation. From this the important conclusion may be drawn that the value of the precessional constant depends almost entirely on superficial data. Former investigators have appealed to the value of that constant as confirmatory of their hypotheses as to the law of internal density, but their arguments now appear to be fallacious.

The same reasoning, however, by which it is concluded that the precessional constant can tell us but little as to the internal density, justifies the use of it as affording the means for an independent evaluation of the ellipticity of the Earth's surface. By this method the author has concluded that the ellipticity must be about $\frac{1}{297}$. M. Callandreau had previously arrived at almost the same value by the same argument.

M. Tisserand, using the best values available when he wrote, was of opinion that the superficial ellipticity is greater than is reconcilable with any law of internal density, if the internal strata are level surfaces. He concluded thence that the central portion of the Earth is more flattened than is consistent with the present diurnal rotation. His work has been confirmed from the mathematical point of view by M. Poincaré; but the author has found that, when the more recent data of Dr. Helmert are used, the apparent discrepancy disappears.

Dr. Wiechert, on the other hand, maintains that there is a

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* Nachr. k. Gesell. zu Göttingen, 1896–97, p. 221.

deficiency of superficial ellipticity; but the author maintains that our knowledge of the Earth's figure is not yet sufficiently accurate to justify any safe conclusion on this point.

Mr. Knobel. I think we all agree that although there may be few present who can enter into a discussion of the masterly paper which the President has presented to us, yet we have the highest admiration of his power in this branch of physical astronomy, of which he is so able an exponent.

Dr. Johnstone Stoney. Am I right in thinking that in all these theories of the figure of the Earth we are proceeding on the hypothesis that the surfaces of equal density are level surfaces? If it is not a figure of revolution it can no longer be a surface of equilibrium.

Prof. Darwin. Undoubtedly the results by Capt. Clarke, and, I think, those of others, do show an inequality in longitude. I think Capt. Clarke put the major axis of the equator about Mid-Atlantic, and that inequality is many times greater than that which the theory indicates as the departure in latitude 45°, namely 3 metres. The only possible way in which one can explain the departure from the figure of revolution is by assuming that there is not complete plasticity. That there is not complete plasticity is obvious, or otherwise we should be under the water at the present moment.

Prof. Turner. In the course of the President's most interesting paper he referred to the researches of M. Callandreau and himself going on parallel lines; but he did not tell us whether those parallel lines have yet met. I gather that this investigation has to do with the discussion of pendulum observations. I do not know whether the President would commit himself as to any opinion on the value of pendulum observations. From a good many conversations with Major Defforges, which occurred while we were doing longitudes in Paris, I am inclined to think that most pendulum observations made up to now are more or less without value, because of the instability of all stands which have been devised. I do not know whether the President has sufficiently examined those observations to be able to say they are more trustworthy. Another interesting point is the precision with which the ellipticity is given. I think we may regard the ellipticity of the Earth's figure as $\frac{1}{300}$ as sufficiently accurate. If the President finds it given with precision to the third figure of the denominator, it is a great advance upon anything we have had hitherto.

Prof. Darwin. With respect to the parallel lines meeting, M. Callandreau does not actually solve the differential equation which gives the departure from true ellipticity of the Earth's surface. Whether he thought that it involved a quantity so small that it was not worth troubling about I do not know. He found, however, a superior limit to the departure from true ellipticity, and showed by an ingenious calculation that the departure from true ellipticity must be less than 6 metres. As to pendulum determination of the ellipticity of the Earth, I cannot say that I have an opinion which is worth giving; but Dr. Helmert, who stands first among geodesists, found from pendulum observations that the ellipticity was $\frac{1}{299}$, and I think he considers that almost a better result than any other; but I would remark that almost all other methods seem to indicate a slightly greater value of the ellipticity of the Earth's surface than that given by pendulum experiments. There is the direct geodetic method, which seems to indicate an ellipticity of $\frac{1}{297}$. Capt. Clarke's work of 1878 gave the fraction $\frac{1}{293}$; but the more recent operations seem to indicate it is somewhat smaller. Then the observed lunar inequality gives a value represented by about 297. I should imagine, so far as I know, that it is extremely improbable that the ellipticity is greater than $\frac{1}{297}$, or less than $\frac{1}{300}$. I rely very largely upon Dr. Helmert's opinion that the most probable value of the denominator is very nearly 298. I do not think we know it within a unit.

A vote of thanks was heartily accorded to Prof. Darwin.

Dr. Johnstone Stoney addressed the Meeting on the subject of the expected appearance of the Leonid Meteors. He stated that since the publication of the paper by Dr. Downing and himself in the 'Transactions of the Royal Society' for March last they had found reason to modify their conclusions, and the maximum of the shower might possibly come some hours, perhaps several hours, before November $15^{d}\ 18^{h}$ *.

In answer to a question by the President, *Dr. Stoney* proceeded to explain the position of the meteor-orbit in space as it resulted from his computations.

The following papers were announced and partly read:—

Rev. T. E. R. Phillips. "Observations of Mars, 1898–99."

J. Gledhill. "Observations of Jupiter and his Satellites made at Mr. Crossley's Observatory, Bermerside, Halifax, in 1898–99."

H. A. Howe. "Observations of Nebulæ made at the Chamberlin Observatory, Colorado."

E. E. Barnard. "On the probable Proper Motion of the Annular Nebula in Lyra."

E. E. Barnard. "The exterior Nebulosities of the Pleiades."

E. E. Barnard. "Diameters of Ceres and Vesta."

Prof. G. H. Darwin. "Theory of the Figure of the Earth carried to the Second Order of Small Quantities."

F. A. Bellamy. "Distribution of Stars Photographed at Oxford for the Astrographic Catalogue for Zones +25° to +29°."

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* The substance of Dr. Stoney's remarks is given on p. 433.

Prof. H. H. Turner. "Variation of Personal Equation with Stellar Magnitude in Meridian Observations at Cambridge, Berlin, and Greenwich, as deduced from Measures of Photographic Plates made at Oxford."

F. W. Dyson and H. P. Hollis. "Comparison of the Diameters of the Images of Stars on the Greenwich Astrographic Plates with the Magnitudes of the Bonn Durchmusterung."

A. C. D. Crommelin. "Ephemeris for Physical Observations of Jupiter, 1899–1900."

H. F. Newall. "Preliminary Note on the Spectrum of *a* Aurigæ."

The following gentlemen were elected Associates of the Society:—

G. E. Hale, D.Sc., F.R.A.S., Director of the Yerkes Observatory, Williams Bay, Wisconsin, U.S.A.

F. R. Helmert, Director of the Geodetic Institute, Potsdam, Germany.

F. Küstner, Director of the Observatory, Bonn, Germany.

Juan M. Thome, Director of the Argentine National Observatory, Cordoba.

The following gentlemen were elected Fellows of the Society:—

Rev. T. G. Barber, B.A., 10 Highfield Road, Doncaster.

S. S. Hough, M.A., Chief Assistant, Royal Observatory, Cape of Good Hope.

The following Candidates were proposed for election as Fellows of the Society:—

Prof. R. N. Apte, M.A., LL.B., Rajaram College, Kolhapur, India (proposed by Sir R. S. Ball).

Lieut. M. H. Clarke, R.N.R., F.R.G.S., Woodlands, Ashburton Road, Croydon (proposed by Capt. P. Thompson).

Rev. P. W. Fairclough, Kaiapoi, New Zealand (proposed by Rev. D. Dutton).

Umes Chandra Ghosh, Muir Central College, Allahabad, India (proposed by A. Mukhopodhyay).

Richard K. Hattersley, 4 Church Terrace, Blackheath, S.E. (proposed by Capt. P. Thompson).

Howard Payn, F.R.G.S., 21 Hyde Park Place (proposed by W. J. S. Lockyer).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The ninth Annual Meeting of the Members of the Association was held on Wednesday, October 25, at Sion College, Thames Embankment, *Mr. W. H. Maw* (the President) in the Chair.

The names of 13 candidates for admission were read and passed for suspension, and the election by the Council of one new member was confirmed.

Mr. Petrie (Hon. Sec.) read the report of the scrutineers of the ballot and the list of officers for the ensuing Session.

The President said all the members had received a copy of the statement of accounts for the past Session, and he, as Treasurer, would be pleased to give any explanations with regard to them that might be desired. On the whole, it was by far the most favourable statement he had the pleasure of submitting. They had written a very substantial sum, over £28, off the value of their stock of journals; they had added £60 to the reserve fund; and they had a surplus income over expenditure of over £63.

Col. Burton-Brown proposed a cordial vote of thanks to the retiring Members of the Council—Mr. Cottam, Sir Wm. Huggins, Mrs. Maunder, and Dr. Isaac Roberts.

The President, upon rising to deliver his address, was enthusiastically received. In opening, he reminded the members that with the present meeting the Association commenced its tenth session, and remarked that inasmuch as it had been his duty as Treasurer during the past nine years to look after the finances, it might perhaps naturally be expected that now, when he addressed them as President, he should say something concerning the progress which the Association had made and the position in which it at present stood. Mr. Maw then pointed to a diagram which he had prepared, showing at a glance the progress which had been made with regard to the number of members, and also how the Association's finances had stood from year to year, from several points of view. Having mentioned that the present membership totalled 1152, he proceeded: "I do not, however, regard these figures as indicating that we have even now closely approached the probable limit of our membership, but rather showing that we should take steps for developing fresh districts in which, as yet, little has been done. The establishment of our provincial branches has always been attended by an increase of membership, and there are several districts, notably in the south and south-west of England, and in Ireland, where such branches ought to meet with substantial support. Our membership, large as it is, for a society of this kind, really represents but a very moderate fraction of those strongly interested in astronomy in this country—a class which, I am glad to say, is, with the spread of scientific knowledge, steadily extending every year. Our Society has now reached such a stage of development that the composition of its membership may be regarded as fairly representative of what it is likely to be for many years to come; and it is thus possible to form a just idea of the work which the Association can profitably undertake, and of the value which such work is likely to possess. Roughly, our members may be divided into three classes—viz., first, the professional astronomers, connected with established observatories, and having at their disposal more or less ample instrumental equipment; second, amateurs having instruments fitted for serious work, and possessing, moreover, a fair amount of experience in the use of such instruments; and third, members who are students of astronomy, but who are either not possessors of instruments at all, or who have not facilities for observational work of an original kind. This third class may possibly be advantageously subdivided, as I shall indicate later on. As regards the first class, the professional astronomers, it is, I am sure, a great satisfaction to us all that we have been able to enrol so many of them in our ranks. A reference to our list of members will show that it includes professional astronomers in nearly every civilized country—men whose names are household words, and the history of whose work constitutes to a great extent the history of the recent progress of our science. It is impossible to overrate the value to our Association of the collaboration of these professional astronomers. Their contributions to our publications may be comparatively few, for, as a rule, the accounts of their researches are, almost of necessity, issued through other channels; but we are indebted to them for some very valuable papers, while we have constantly benefited by their presence at our meetings and by their aid in various ways. Without them the status of our Association would certainly not be what it now is. Next, as to the second class, that consisting of amateurs possessing more or less excellent instrumental equipments. It is this class which has formed the backbone of our observing sections, and to it is due the bulk of the original work which has been recorded in the publications of the Association. . . . . It is satisfactory to know that this second class of our members, upon whom we depend so much for original work, is an ever-growing one, while year by year it is becoming possessed of more powerful and more perfect instruments. And this brings me to a point on which I wish to say a few words—namely, the influence of the large telescopes, with which some of our more important observatories are now provided, on the usefulness of the work of amateur observers. In considering this point it must be borne in mind that the great telescopes of the present day are of a very different class to the giant instruments of the past. The great reflectors of Herschel, of Lassell, and of Rosse were all instruments made and used by amateur astronomers, and although they possessed great light-grasp and did some admirable work, they were, owing largely to the nature of their mountings, utterly unfitted for the class of observations on which the large telescopes of the present day are chiefly employed. Our modern giant telescopes are, with few exceptions, employed not in doing work which was formerly done by smaller instruments, but in doing work which formerly could not be done at all. Such, for instance, is the bulk of stellar spectroscopic work, including determinations of velocity in the line of sight, the measurement of close double stars, the spectroscopic examination of nebulæ, the discovery of new planetary satellites, and similar matters. We see, therefore, that the establishment of these powerful telescopes has been accompanied by the development of new fields of research, and that the work which was formerly done —and can still be well done—by instruments of moderate size has not been reduced. On the other hand, many professional astronomers have withdrawn from the work which they formerly did with the instruments then available, and they have thus left to amateur observers the continuance of their former labours. We thus see that there is ample work for the members forming our observing sections, and that such work, if faithfully carried out and recorded with judgment and discrimination, is calculated to be of great and permanent value."

The President next dealt at some length with the character of the reports of the observing sections, and with the mode of recording the work of those sections, in which connection he suggested some improvements which might be made. He continued: "I have now to deal with the third class of our members, namely, those who are either non-observers, or who, if they observe at all, are provided with a very limited instrumental equipment. This is a very large and important class, and it may, as I have already hinted, be conveniently subdivided into at least two sections—one comprising those who already possess a considerable knowledge of astronomy, and the other consisting of those who are more or less beginners in the study of our science. To both these sections our Association should be of considerable service, while both, on the other hand, can materially aid the objects which the Association has in view. To our members forming the first subdivision much really useful work is open. In the first place, they may render valuable aid to our observing sections. . . . Then, again, there are other directions in which the class of members with whom I am now dealing can do useful work. Some may strike out original lines of mathematical or geometrical investigation, as has been done by Mr. Whitmell, whose papers contributed to our 'Journal' aid us so much in realizing aspects of the Solar System regarded from other planets than our own; others may take up optical matters and assist in the perfecting of our telescopes and spectroscopes, while still others may afford to our hard-working Editor much-needed assistance in his preparation of those abstracts of foreign astronomical publications which form so valuable a feature in our 'Journal.' I have, however, I think, said enough to show that the fact of not possessing an observatory or instrumental equipment is no bar to the accomplishment by competent members of work of real value to our Association and to astronomical science generally. I have now finally to deal with the second section of the third of the three classes into which I have ventured to divide our members. This section, it may be remembered, consists of those who are commencing the study of astronomy. I will not call these members 'learners,' because that is really not a distinctive term. An astronomer devoted to his science never ceases to be a 'learner' however eminent he may become, and, in fact, with the growth of knowledge comes inevitably the growth of the conviction that great as has been the progress of astronomical discovery we have as yet only touched the fringe of that great science whose possibilities are as limitless as space itself. Using, then, for the want of a better, the term 'beginners' to denote the class of members of which I am now speaking, I wish before concluding this address to offer a few remarks on the manner in which I consider that the study of astronomy can be most advantageously commenced. . . . . I trust that the remarks which I am about to make on the commencement of the study of astronomy will not lead anyone to suppose that I in the least underrate the value of the numerous popular works on our science, or the admirably illustrated magazine articles dealing with astronomical subjects, of which so many have appeared during recent years. On the contrary, I believe such books and articles have done great good, and have, by the interest they arouse, caused many additions to be made to the ranks of amateur astronomers. But the beginner who, wishing to study astronomy, confines his attentions solely to such writings as I have just referred to is much in the position of a man who thinks he can become a soldier by reading glowing accounts of hard-won victories. Such a beginner, who has had his imagination stirred by the examination of beautifully reproduced photographs of comets, or nebulæ, or lunar views, is apt to experience more or less severe disappointment when he is shown these objects through a telescope of moderate size. Not having had experience in observing, he misses much detail which even such an instrument can show, and realizing how far what he sees falls short of what he had been led to expect, he is apt to jump to the conclusion that observational astronomy, at all events, offers few attractions to those who have not at command an expensive instrumental equipment. Now, this conclusion is an utter mistake—a fact which the beginner who approaches the study of astronomy in the proper spirit will soon recognize. It has to be borne in mind that, great as are the attractions of modern astro-physical research, the real basis of our science is that which is sometimes called, by way of distinction, 'gravitational astronomy.' It has, further, to be borne in mind that the earlier astronomers working with instruments of a very elementary kind obtained a considerable knowledge—which was in many respects really remarkable for its approximate accuracy—as to the motions of the heavenly bodies, and as to the phenomena presented by the chief members of our Solar System. Now, what was done in the olden times can be done in the present day; and I wish to prominently direct the attention of beginners to the fact that by the employment of quite simple apparatus they may make observations which will bring home to them, in a way which mere reading can never do, a knowledge of many astronomical phenomena which they will find to be, not only of immediate interest, but of great value to them in their further studies. What I wish to urge, therefore, is, that those commencing the study of astronomy should not be content with reading only, but should work in the open air, faithfully and systematically recording their observations, however elementary these may be. I lay great stress on this latter point, because unrecorded observations have, as a rule, little educational value. The mere fact of describing in writing any observation, however simple, which has been made is of immense assistance in securing completeness and accuracy. .... To the beginner who has thus taken up the study of celestial motions an endless number of problems will suggest themselves for examination, and it will be found that the solution of these problems will afford work which is not only of great immediate interest, but will lead to the acquirement of knowledge of considerable future value. It has been often said that 'learners should not be ashamed to ask questions.' This is quite true in a certain sense, and no beginner should be ashamed to acknowledge that he has much to learn. But the practice of asking questions is not one to be advocated, except within certain strict limits. The beginner who immediately he gets into a difficulty asks for aid to get out of it, is not likely to make any great progress. It is the battling with difficulties, the habit of regarding a problem from various points of view, and the practice of 'getting at the bottom of things,' which impresses truths and principles on the mind, and a few facts so learned are worth ten times the number acquired by the question and answer method. The statement I have just made appears to me to apply with special force to the use of instruments. Many present have no doubt been struck, as I have, by the character of numerous queries respecting the use and adjustment of instruments which, from time to time, appear in print. These questions suggest the idea that those proposing them are of opinion that scientific instruments should be made on the 'You-touch-the-button-and-we-do-the-rest' principle, and that their employment should require no special knowledge on the part of the user. Now, this is a frame of mind which is much to be deprecated. Nothing is more essential to secure the best results with any instrument than a clear comprehension of the principles on which such instrument is constructed. It is only by the possession of such knowledge that the user of a telescope, a spectroscope, or other astronomical appliance can determine whether or not any defect in performance is due to a radical fault in such instrument or to a comparatively trivial fault in adjustment. For this reason I would thoroughly urge beginners, when they take up actual observing, to study carefully the theory of any instrument they may employ, and make themselves familiar with its principles and construction. Were this more generally done, much disappointment and loss of time would be saved, and instrument makers would be spared many unjust complaints and much worry. I am afraid that my remarks on the section of our members which I have classed as 'beginners' have run to an undesirable length; but it must be remembered that the 'beginners' of to-day are those from which we shall at an early date expect work which will promote the interest and strengthen the position of our Association, and any suggestions which may aid in their training may thus possibly be regarded as excusable. In conclusion, I may quote a passage from the works of Bacon, which was written to have a wide significance, but which appears to me to apply with peculiar force to the science to which we are all devoted. Says the great philosopher: 'Knowledge is not a couch whereon to rest a searching and restless spirit; nor a terrace for a wandering and variable mind to walk up and down with a fair prospect; nor a tower of state for a proud mind to raise itself upon; nor a fort for commanding ground for strife and contention; nor a shop for profit or sale; but a rich storehouse for the glory of the Creator and the relief of man's estate.' "

A vote of thanks to the President for his brilliant address was proposed by *Mr. E. W. Maunder* and accorded with enthusiasm by the Meeting.

A paper by *Mr. Cecil J. Dolmage*, on "The Apparent Enlargement of Heavenly Bodies when in the Neighbourhood of the Horizon," was read by *Mr. Maunder* and gave rise to a spirited discussion, in which Mr. Maunder, Mr. McCarthy, Mr. Wesley, Mr. Nelson, Mr. Clapton, and Mr. Tappenden took part.

Mr. Petrie read a paper by *Mr. Edwin Holmes*, which consisted of a sketch of the life and work of John Bird, who in the early part of this century visited nearly every town in the kingdom as a lecturer on astronomy.

ROYAL METEOROLOGICAL SOCIETY.

The opening Meeting of this Society for the present Session was held on Wednesday evening, November 15, at the Institution of Civil Engineers, *Mr. F. C. Bayard, LL.M.*, President, in the Chair.

Mr. R. H. Curtis read a paper on "The Diurnal Variation of the Barometer in the British Isles." The principal features of a curve exhibiting the diurnal march of barometrical pressure are two minima and two maxima—the first minimum occurring early in the morning and the second in the afternoon, while the first maximum falls in the forenoon and the second not far from ten o'clock in the evening. In the tropics the oscillation may amount to as much as a tenth of an inch, but its amplitude decreases as the latitude increases, and the greatest amplitude in the British Isles amounts to not much more than three-hundredths of an inch. The author discusses the mean hourly readings of the barometer from 25 years' observations, 1871–95, at four observatories maintained by the Meteorological Council, viz., Kew, Aberdeen, Falmouth, and Valencia. The author is of opinion that the primary cause of the diurnal oscillation of the barometer is solar radiation, and that its amplitude is chiefly determined by the temperature of the lower strata of the atmosphere. The relative magnitudes of the different phases of the barometer oscillation, as observed, depend largely upon the geographical position and physical surroundings of the place of observation, in so far as these are capable of modifying its temperature conditions, and especially the relative distribution of temperature over the regions immediately surrounding it.

Mr. G. J. Symons, F.R.S., described some experimental observations which he made during the hot weather in July with two thermometers one foot below the surface of the ground, with the view of ascertaining (1) the influence of slight shade, (2) the amount of daily range, and (3) the approximate curve of daily fluctuation.

The Leonids of 1899.

THE long-looked-for time when a repetition of historic meteor displays was expected has come and gone, leaving behind it a feeling of disappointment; a disappointment perhaps more felt by the general public than by astronomers, who were to some extent prepared for a non-appearance of the Leonid-shower, as had happened in 1897 and 1898.

The daily newspapers and the magazines had created a popular interest in the subject, without adequate cautions as to the uncertainty of the event, and as the result many thousand persons assembled in open spaces in London, and no doubt in other parts of the country, on the night of November 14 and 15 for observation of the shower which did not happen. An incident, almost tragic, was the experience of the Rev. J. M. Bacon and his daughter, who made a balloon ascent from Newbury, Berks, and came to earth again, after a more than exciting voyage, at Neath, in Wales, so roughly that Miss Bacon was severely injured and her father badly shaken. The record of the comparatively few observations, extracted in some cases from Mr. Denning's report in 'Nature' of Novemder 23 is as follows:—

On November 6, 8, 10, and 11, Prof. Herschel at Slough, Mr. Besley at Clapham, Mr. Astbury at Wallingford, and

Mr. Denning at Bristol observed meteors in varying numbers; but of these there were only two on the 8th, two on the 10th, which might be Leonids, and these perhaps did not actually belong to the stream. On the 11th two certain Leonids were seen at Bristol, which indicated the position of the radiant at 152° +23°. The 12th was much clouded, but on the 13th seven Leonids were seen at Croydon, one at Bristol, eighteen at Marlborough, and twenty-three at Cambridge.

On the night of the 14th a dense fog came up in London about midnight, and nothing could be observed at Greenwich. At Wallingford, Mr. Astbury saw twenty-five Leonids, Sir W. J. Herschel at Littlemore, near Oxford, observed sixty-six Leonids and thirty-two other meteors during the night. At Worthing, Mr. Schulz counted more than 200 Leonids between 15^h and 18^h. At Oxford about fifty in the early morning. On the night between the 15th and 16th the sky over London was covered with light cloud, and nowhere were more Leonids seen than in an ordinary year. At Greenwich seventeen Leonids were seen in a cloud-break of 42 minutes, about 6 o'clock in the morning. At Cambridge a hundred meteors were observed between midnight and half-past four.

Reports from abroad are meagre. They say that at the Treptow Observatory, Germany, a large number of meteors were seen between 3 and 5 o'clock in the morning of Thursday, November 16. At Berlin thirty-one Leonids were seen about half-past 6 on the same morning. From Vienna it is reported that thousands of persons spent three nights on a hill near the city, in the hopes of seeing this display, but the weather was quite unfavourable. A party who went to the Sonnwendstein saw about one hundred and thirty-five Leonids on the morning of the 15th. Dr. Weiss of Vienna went to Delhi with an observing party: telegraphing at noon on the 16th, he said "Leonids not yet appeared."

From America the reports are equally unsatisfactory, the weather being generally unfavourable. Sixty-nine Leonids were observed from Pennsylvania (the time is not given), and thirty were seen at Chicago just before daylight of the 16th, three of which were particularly bright.

As has been said, astronomers were in some measure prepared for the non-appearance of the meteors. The researches of Drs. Johnstone Stoney and Downing, communicated to the Royal Society in March last, gave November 15^d 18^h as the most probable time for the middle of the shower; but at the meeting of the Royal Astronomical Society on November 10, Dr. Stoney gave the result of further researches, which made the prediction less certain. This result was published in various newspapers. The following is abstracted from a letter by Dr. Stoney to the 'Times' of November 14:—

"During the great shower of 1866, the position of the point of the heavens from which the meteors appeared to diverge was very carefully observed, and this determination, combined with what Professor Newton had brought to light, enabled the late Professor J. C. Adams to make one of the most brilliant astronomical discoveries of the present century—viz., the discovery of the actual orbit within the Solar system which was being traversed by the meteors which are situated in the neighbourhood of that part of the stream through which the Earth passed in 1866. ....

"The actual shift of the node—that is, the amount by which the meteoric orbit has moved sideways along the Earth's orbit—has had considerably more than three times its average amount in consequence of the intense perturbation to which the meteors have been subjected within the last 33 years; and this is not the only respect in which Adams's orbit has been removed from the position in which he found it in 1866. This meteoric orbit then intersected the Earth's orbit. It has since been forced away from the Earth's orbit by the disturbing action of the planets, chiefly of the great planets Jupiter and Saturn. In the position which it now occupies the Earth will pass closest to it upon next Thursday morning, at about 6 o'clock A.M.; and Adams's meteoric orbit will then be as much as 1,300,000 miles distant from the Earth's orbit, that is more that five times further off than the Moon.

"This would be quite enough to carry the stream of meteors quite clear of the Earth if that stream were a mere cylindrical stream like a thread travelling lengthwise through space. But the investigations which we have had to make have brought to light the important fact that the stream where it pierces the plane of the Earth's orbit is not like a thread but more like a long piece of strap or tape travelling forwards in the direction of its length—in other words, the stream is very much wider than it is thick. Its thickness is known, from the duration of the meteoric showers, to be about 100,000 miles; but all that is known of its width is that it is much more. We shall probably not be far wrong if we estimate it as being three or four millions of miles. This very wide and comparatively thin stream of meteors passes obliquely through the plane of the Earth's orbit, and the part of the plane through which it passes has obviously the shape of a long oval or rectangle. We know where one point in this oval is at present—namely the point spoken of above where Adams's orbit pierces the plane of the Earth's orbit—a point which, as we have seen, will be 1,300,000 miles nearer to the Sun than the position which the Earth will reach at 6 A.M. on next Thursday morning. Unfortunately we do not know the length of the longer axis of the oval section, nor its position, further than that its direction originally, that is, 17 centuries ago, lay perpendicular to the Earth's orbit, and that it has since been very slowly shifted by perturbations into a position which slopes towards a part of the Earth's orbit which the Earth will reach sooner than 6 o'clock A.M. on Thursday morning. When the shower of this year takes place, if there is one of the great showers this year, we shall know from the time of its occurrence how much the oval section has turned out of its original position. Meanwhile we can only say that the shower is to be expected before 6 A.M. on Thursday, probably some hours earlier, and possibly, but not probably, so much earlier that the beginning of the shower may be seen from our side of the Earth before dawn on Wednesday morning. It is, however, more likely to come during the daytime or evening of Wednesday, in which case it will not be seen in England, or after 10.30 P.M. on Wednesday evening, in which case it will be visible if the weather permit."

Variable Velocities of Stars in the Line of Sight.

It will be remembered that last year Professor Campbell, of the Lick Observatory, announced that the stars η Pegasi, o Leonis, χ Draconis, ζ Geminorum, ι Pegasi, and θ Draconis showed variation in radial velocity. Almost at once confirmation was given by Prof. Belopolsky, of the Poulkova Observatory, for the cases η Pegasi and ζ Geminorum.

In the October number of the 'Astrophysical Journal,' which has just appeared, Professor Campbell announces variable velocities in the case of no less than seven stars—α Aurigæ, ϵ Libræ, h Draconis, λ Andromedæ, ϵ Ursæ Minoris, ω Draconis, and α Ursæ Minoris. He devotes a special note to Capella, giving results derived from photographs obtained in 1896–97. The spectrum is described as composite; in one photograph it is of essentially normal solar type, in others there appear signs of a second component, whose spectrum contains the H_γ line and the prominent iron lines. The two spectra are superposed, the lines of similar origin being sometimes coincident, sometimes relatively shifted, so that they appear to be ill-defined or double. The period of the binary system is as yet not determined.

A second special note deals with the case of Polaris. Professor Campbell found about a year ago, from six photographs taken between Sept. 8 and Dec. 8 in 1896, that the velocity appeared to be constant, $-19{\cdot}6$ kilometres per second. In August 1899 another photograph was obtained of the same spectrum to test the current work of the spectrograph, and a velocity $-13{\cdot}1$ kilometres per second was deduced. Variability was suspected, and after 14 more photographs had been secured, variability was established and a period of $3^{d}\ 23^{h}$ was found. "The velocity at present seems to be included between $-8{\cdot}6$ and $-14{\cdot}6$, having an extreme range of only 6 kilometres per second." To reconcile the present velocities with those determined for 1896, Professor Campbell does not hesitate to state that he has no doubt that the velocity of the binary system is changing under the influence of an additional disturbing force; in fact he regards Polaris as at least a triple system. This, however, is a conclusion which seems hardly justified by the evidence given in the note; for, in face of the statement that the recent photographs of Polaris were taken in order to test the current results of the spectrograph, it is obviously necessary to show by comparison of results, in the case of a star whose velocity may be relied on as constant, that the current results are consistent with the earlier.

Prof. Frost's short note, in the same number of the 'Astrophysical Journal,' summarizing the results obtained at the Yerkes Observatory, tend to corroborate the existence of a variation of velocity in the case of Polaris, and also to emphasize the need for the further evidence above referred to; for whilst he gets practically the same range of variation (viz. 7 kilometres per second as compared with Campbell's 6 kilometres per second), the absolute values of the velocity fall midway between Campbell's values for 1899 and those for 1896.

It will be evident from these brief notes that the determination of radial velocities has entered upon more certain courses. The beautiful samples of stellar spectra that have quite recently been received at the Royal Astronomical Society from the Lick Observatory are evidence of the splendid material which Prof. Campbell is securing for his determinations. But the surest test of accuracy is to be found in the corroboration of his results afforded by independent workers, and in this connection I am glad to be able to confirm from my own observations Prof. Campbell's discovery that Capella is a spectroscopic binary; for, before his note reached us in this country, I presented a short preliminary note to the Royal Astronomical Society at its last meeting to announce the discovery of the binary nature of the star as one of the first results obtained with the new four-prism spectroscope attached to the 25-inch equatorial of the Cambridge Observatory. I feel sure that Prof. Campbell will agree that there is enough work for at least two workers in deciphering the complexities of the spectrum of this new binary. H. F. Newall.

The Crossley Reflector *.

The reflecting telescope has been so little used in this country, since the time of Henry Draper, that a few notes on the photographic efficiency of the Crossley reflector may be of interest to American readers.

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* [This instructive article by Prof. Keeler is taken from the 'Publications of the Astronomical Society of the Pacific,' vol. xi. no. 70. It will be remembered that this 3-foot mirror, originally made by Calver, was used by Dr. Common to obtain the photographs for which he received the Gold Medal of the R.A.S. in 1884. It was afterwards acquired by Mr. Crossley, who presented it in 1895, after it had undergone some alteration, to the Lick Observatory, where it was the cause of some dissension. See this magazine, vol. xx. pp. 296, 299.—Eds.]

The Crossley dome is built on the farther end of a long rocky spur, which extends from the principal summit of Mt. Hamilton towards the south, and is within a few minutes' walk (or climb) of the main Observatory. The mirror, which has a very fine figure, has an aperture of three feet, and a focal length of seventeen feet six inches. The mounting, as compared with the beautifully mounted large refractors of the Observatory, is undoubtedly a rude piece of mechanism, but with sufficient experience of its numerous idiosyncrasies the observer can obtain negatives with exposures of four hours' duration, with only an occasional failure.

At present the Crossley telescope is being used for photographic observations of nebulæ. For such work the summer months at Mt. Hamilton present almost ideal conditions. The sky is continuously and often brilliantly clear, while the dryness and purity of the air are such that the silvered surfaces retain their brilliancy without any care on the part of the observer. Within the last week, however, the smoke from forest fires (from which there seems to be no escape in even the remotest corners of the Earth) has greatly dulled the brightness of the sky, and has interfered most annoyingly with the photographic work. In the winter months, on the other hand, the conditions are generally bad, on account of storms, snow, fog, or dampness; yet there are many nights, between the spells of bad weather, on which the telescope can be used.

To one who, like myself, has always worked with refracting telescopes, the photographic power of a large reflector is surprising. In this respect, the Crossley reflector does not, of course, surpass any other reflecting telescope of like dimensions, but its photographic "rapidity" is certainly very considerably greater than that of a refracting telescope of the same angular aperture. This is due to the fact that the silvered surfaces absorb less of the chemically active light than the glass lenses of a refractor, and it is noteworthy that this superiority of the reflector becomes more pronounced the finer the atmospheric conditions under which the two classes of instruments are compared. On one of the fine nights which I have mentioned, when the Milky Way shines with astonishing splendour and the whole heavens look phosphorescent, the photographic activity of the reflector is remarkably increased. But the performance of the refractor is not greatly changed, for the reason that the short light-waves, which are transmitted more abundantly by the unusually clear air, are in any case strongly absorbed by glass.

To illustrate the photographic rapidity of the Crossley telescope, I give the following examples of exposure-times in which well-known nebulæ have been photographed:—

The Ring nebula in Lyra has been photographed on several occasions, and the results are described in another part of the present number of the Publications. It will be seen that the best general representation of the nebula was obtained with an exposure of ten minutes. On this plate the stars are perfectly round and very small. The disc of the central star of the nebula has a diameter of 3″·5; that of the smallest stars shown does not exceed 1″·5. On the same plate is a double star (not resolved) the equal components of which are about 2″ apart, while their magnitude is not less than 17 or 18. It will hardly be observed visually. The central star, which has a visual magnitude of 15·4, according to Burnham, gave a distinct image in one minute.

Photographs of small planetary nebulæ have been made, not only for the purpose of ascertaining the exposure-times required for such objects, but to see what amount of detail can be shown in a surface of such small dimensions. With regard to the latter purpose, it was found that a large amount of detail was shown, but that visual observation with the 36-inch refractor was more satisfactory than photographic observations with the reflector.

In the case of the small but remarkable planetary nebula G. C. 4628 (26″ × 16″), the best general picture was obtained in two minutes, while the "ansæ," extending outward from and connected with the main nebula, were well shown in ten minutes.

Another planetary nebula which has been photographed is G. C. 4964. Eight images were obtained on the same plate by slightly changing the position of the plate-holder between the exposures, which ranged from two minutes to one second. The images which received exposures of one minute, thirty seconds, and twenty seconds respectively, were the best. A weak image, in which the central star was just visible, was produced in two seconds, and a bare trace of the nebula was visible at the place where the exposure was one second.

The planetary nebula in Draco, G. C. 4373, has also been photographed, with quite similar results; and the small nebula discovered by Professor Barnard near the star Merope is distinctly shown on a negative of the Pleiades which was exposed for thirty seconds.

The photographic power of the reflector which is illustrated in the foregoing examples is very advantageous in the case of objects which are of very unequal brightness, as a very full light-action, and consequently softening of contrasts, is obtained with a comparatively short exposure. If, for instance, a plate is exposed for ten minutes to such an object as the great cluster in Hercules, and rather strongly developed, a negative is obtained which shows very distinctly all the brighter stars. If the plate is exposed for two hours, and is then very lightly developed, the brighter stars appear much as before, but the swarms of minute stars, to which a globular cluster seems to owe its nebulous aspect, also appear on the plate, so that the photograph closely represents the photograph of the cluster as seen in a large telescope. On a photograph of the cluster in Hercules, made with the Crossley reflector on July 13th, with an exposure of two hours, over 5,400 stars were counted within the limits of the cluster. The average diameter of a star disc is 3″·5. A discussion of the distribution of stars in the cluster, as shown by this photograph, has been made by Mr. Palmer.

With exposures of four hours, stars and nebulæ are photographed which are far beyond the range of the 36-inch refractor. On one plate (3¼ × 4¼ in.) sixteen new nebulæ were found. It would be easy with this instrument greatly to increase the number of known nebulæ; but the discovery of new nebulæ, all of which would necessarily be faint, seems to be much less important than the gain of further information about nebulæ already known. For this reason no search has been made for new objects, though a catalogue will be made in due time of those which have been found in the course of other investigations.

J. E. K.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Rotation of Saturn.

Gentlemen,—

As some modern astronomical books continue to give the duration of the rotation of the ringed planet nearly a quarter of an hour too long, it may be worth while to devote a few words to the earliest instances of this error, which I have at last succeeded in tracing to its origin, as I think will be allowed.

In Houzeau's 'Vade Mecum' $10^h\ 16^m$ is given as the actual time determined by W. Herschel, and a note is appended that the $10^h\ 29^m$ in Baily's 'Astronomical Tables and Formulæ' (1827) is not really Herschel's, probably meaning that it is incorrectly quoted.

In Proctor's 'Old and New Astronomy' (p. 624) Herschel's period is given correctly in the text, and a footnote says: "A rotation period of 10 hours 29 minutes 16·8 seconds is said in several old treatises to have been Herschel's final determination. There must be some authority for a statement so definite and precise. Prof. Newcomb says that 'a suspicious coincidence is that this period agrees with that assigned for the time of the rotation of the ring,' but this is not the case. [The period found by W. Herschel for the rotation of a certain part of the ring was $10^h\ 32^m$.].... The period 10 hours 29 minutes 16·8 seconds is given in the old *Penny Cyclopædia*, in Rees's *Cyclopædia*, also old, and in Smyth's *Celestial Cycle*, nearly half a century old. I have been unable, however, to trace this period to its source."

Proctor is wrong as regards that very useful production in its day, Rees's 'Cyclopædia,' for Herschel's period is there correctly given, as it is also in Vince's 'Complete System of Astronomy'

(1797). The erroneous value does indeed appear in the 'Penny Cyclopædia' (1841) and in the 'Celestial Cycle' (1844). Like Prof. Newcomb, I have looked through all Herschel's papers for this express purpose, and can find no such result. What is most extraordinary about the matter is that it appears not only in Sir John's treatise on Astronomy (1833), forming part of Lardner's 'Cabinet Cyclopædia,' but in all the successive editions of the 'Outlines,' founded on that treatise. My conjecture that Sir John inadvertently copied the duration at first from pp. 39 and 67 of Baily's 'Tables' (we all know how difficult it is to get rid of an error once committed) is now fully confirmed, as will be seen presently.

Sir W. Herschel, in communicating his result (Phil. Trans. for 1794), confidently expresses the opinion that it is much more than two minutes in error; and it is interesting to notice that that of Prof. Asaph Hall (obtained in 1876), which amounts to $10^h\ 14^m$, is two minutes less than Herschel's. The latter has several later papers on the peculiar form sometimes presented by the planet, but does not appear to have recurred to the subject of its time of rotation. Schröter, before the publication of Herschel's determination, made some rough estimates; his paper forms one of the 'Schriften' of the Berlin 'Gesellschaft naturforschender Freunde' for 1792.

Baily, then, must have been the originator of the error in question, and I believe I can show how he fell into it. He tells us in his preface that he took the "Elements of the Solar System" from the fifth edition of Laplace's 'Exposition du Système du Monde.' Now, turning to that, we find that the numbers are given in fractions of a day. The rotation of Saturn is stated (p. 79) to have been determined by Herschel (erroneously spelt Herschell) at 0·428 day, which is as near $10^h\ 16^m\ 0^s$ as can be given to three decimal places, and is therefore quite correct. But on the previous page (78) the revolution of the part of the ring observed by Herschel is stated to be 0·437 day, which corresponds to the 10 hours 29 minutes 16·8 seconds given by Baily for the rotation of the planet. Laplace evidently made a slight mistake in the last decimal, which should be 9, for 0·439 day corresponds to 10 hours 32 minutes, the value really found by Herschel for the revolution of part of the ring.

Thus, then, the source of the erroneous value, $10^h\ 29^m\ 17^s$, of the rotation is fully explained. Its appearance in a work so often quoted and referred to as Mr. Chambers's 'Handbook' (not to mention others, such as Dunkin's edition of Lardner) justifies me in asking your permission to call attention to it in the 'Observatory.' Prof. Newcomb's conjecture was essentially right after all.

Yours faithfully,

Blackheath, 1899, Nov. 1. W. T. Lynn.

Proper Motion of the Markings on Jupiter.

GENTLEMEN,—

It is well known that in different latitudes of Jupiter there are currents, forming the belts and zones, moving at various rates of speed. In many instances the velocity changes from year to year. And it is a singular circumstance that in the same current a uniform motion is not maintained in all parts of the circumference. Certain spots move faster than others, so that if we would obtain a fair value for the rotation period of any current it is not sufficient to derive it from one marking alone; we must follow a number of objects distributed in different longitudes along the current, and deduce a mean from the whole. Who first discovered the irregularities referred to it is difficult to say, but it is certain that it could not long escape notice after the telescope was sufficiently improved to show the principal markings visible on the planet. J. D. Cassini, more than two centuries ago, found that the spots exhibited remarkable proper motions. W. Herschel and Schroeter, a century later, re-observed similar phenomena. Mädler in 1834–5 followed two spots in the same latitude during several months, and found a slight displacement in their relative positions. In 1858–9 Baxendell called attention to some spots in the S. tropical region of Jupiter, which exhibited a difference of rotation period to the extent of seven seconds, though placed in approximately the same latitude. During the last twenty years Mr. A. S. Williams has several times called special attention to the unequal velocity manifested by spots in the same currents, and has suggested that the rotation period should always be determined from as many objects as possible, so that the mean motion may be fairly represented.

I have also given some attention to this subject, though my observations of the planet have been intermittent. In 1898 I followed 23 equatorial spots, and found a mean period of $9^h\ 50^m\ 23^s{\cdot}6$, while the individual markings gave values ranging from $9^h\ 50^m\ 16^s{\cdot}9$ to $9^h\ 50^m\ 33^s{\cdot}2$. In 1899 I watched 27 equatorial spots, and obtained a mean of $9^h\ 50^m\ 24^s{\cdot}6$ from them all. The extremes were $9^h\ 50^m\ 18^s{\cdot}6$ and $9^h\ 50^m\ 35^s$. In 1898 three N. tropical spots indicated periods of $9^h\ 55^m\ 24^s{\cdot}8$, $9^h\ 55^m\ 26^s{\cdot}3$, and $9^h\ 55^m\ 27^s{\cdot}7$. In 1899 sixteen N. tropical spots gave a mean period of $9^h\ 55^m\ 28^s{\cdot}8$, but the individuals showed surprising differences: thirteen of the spots were represented by a mean motion of $9^h\ 55^m\ 32^s{\cdot}4$, while three of them, between longitude 150° and 255°, had a rate of $9^h\ 55^m\ 16^s{\cdot}4$. It is clear from these figures that near the equator and in the N. tropical zone the individual markings vary to the extent of at least 16 or 17 seconds in their rotation periods. This sufficiently asserts the necessity of following as many objects as possible to ensure accurate results, for a rotation period derived from a single marking can only be said to represent that marking, and may be widely different to others circulating in the same longitudinal current.

Yours sincerely,

Bristol, 1899, Oct. 20. W. F. DENNING.

The Solar Motion.

GENTLEMEN,—

The point by Mr. Backhouse in the 'Observatory' for November, p. 396, seems to me well made. "Linear speed" should have been the term used. It would be hardly necessary to add the point of reference, as this is well understood, but very complicated to express.

Yours respectfully,

Washington, 1899, Nov. 13. S. NEWCOMB.

OBSERVATORIES.

[Continued from p. 400.]

UTRECHT. *A. A. Nijland.*—Variable stars, β Lyræ, δ Cephei, η Aquilæ, and o Ceti. Drawings of Jupiter, observations of Satellites. Observations of meteors; Comets 1898 I., Perrine-Chofardet, and Brooks. Time service.

VIENNA (*M. Edler v. Kuffner*). *L. de Ball.*—Variable stars β Lyræ, η Aquilæ. The results show a close agreement between the visual and photographic light curve in the case of β Lyræ; but in the case of η Aquilæ there is a marked difference in position of maximum and minimum. Photographs of Pleiades, Præsepe, the clusters λ and χ Persei; and researches on photographic magnitudes generally.

ZÜRICH. *C. Wolfer.*—Sun examined on 265 days, prominences being noted on 140 days. The sun-spot observations have been continued, the results of the last two years being:—

	Relative no.	No. of days of observation.	Days free from spots.
1897	25·9	270	21
1898	26·2	273	30

Time service.

THE U.S. NAVAL OBSERVATORY.—It will be remembered that this institution has been the subject of considerable criticism in recent years, both as to its administration and on account of its output of work, so much so that in June last the Secretary of the U.S. Navy instructed a Board of Visitors, consisting of three well-known astronomers and two other gentlemen, the Hon. W.

E. Chandler and the Hon. A. G. Dayton, to examine into the condition of the observatory. This Board has lately presented its Report.

The main reason of the trouble appears to have arisen in this way. The Naval Observatory, which was originally established as a scientific bureau auxiliary to the needs of the naval service, has become through half a century of growth, and through the expenditure of large sums of money, as authorized by law, an astronomical observatory of the first rank in respect of buildings and equipment. But though it has thus changed, and the greater part of the equipment has little direct reference to the requirements of the Navy, it still remains under that Department. So far the case is analogous to that of Greenwich; but, unlike the English observatory, it is officered by naval men, and there is not a professional astronomer in supreme command of the place. Under this system no Director has remained long enough in office, nor has there been any plan for conducting a series of researches with regularity over a long series of years, which constitutes the great advantage of a National Observatory. Each principal astronomer seems to have been left to choose his own time of work, and to alter it or abandon it from time to time. Further, the work of the Nautical Almanac Office has lately been attached to the Naval Observatory, and Prof. Harkness during the last year or two has been in charge of both offices, under a naval officer as chief. The work of the observatory and the 'Nautical Almanac' is evidently too much for one man.

To remedy this state of things the Board of Enquiry recommend the appointment of a permanent Board of Visitors, who shall make an annual report to the Secretary of the Navy, and shall prepare regulations prescribing the scope of the researches to be pursued at the observatory. Further, the Board of Visitors shall recommend persons as they are required for the offices of Astronomical Director, Astronomer, or Assistant Astronomer—the scheme of the Board, stated or implied, being that the present staff, who are entitled "professors of mathematics in the U.S. Navy," shall continue to hold their offices, but as vacancies arise they shall be filled by astronomers from civil life, and a schedule of salaries is proposed on a sufficiently liberal scale to attract men of the highest class. The Board further recommends the separation of the Nautical Almanac Office from the Observatory. A note in 'Popular Astronomy' makes the very apt suggestion that the name should be changed; the word "National" being substituted for "Naval."

PERTH OBSERVATORY, WEST AUSTRALIA.—Mr. W. E. Cooke, the Director of this lately-established Colonial Observatory, has begun to issue a series of publications. We have recently received two small instalments. The first contained meteorological observations for the year 1897; the second, as might be expected from a new observatory, was a report on the determination of its terrestrial coordinates. The difference of longitude between Perth and Adelaide Observatories was determined by transmission of telegraph-signals on five nights, the first of which was March 30, the last August 2 of the present year. Clock-errors were determined at Perth by Mr. Cooke with a 6-inch transit-circle, and at Adelaide by Mr. Griffiths, with, it is assumed, the transit instrument of the observatory. From the transmission of signals in the two directions, eastward and westward, it is deduced that the time of transmission was $0^{s}\cdot26$, which, at first sight, seems large for a distance of about 1500 miles of land-line; but it may be accounted for by the occurrence of several relays in the circuit. In connection with this point an interesting experiment was made. The circuit from the sidereal clock at Perth sets in action a multiple arm relay: one of the issuing circuits recorded clock-beats directly on the chronograph; another of the circuits was extended to the Post Office, and, after passing along a line including Albany, Eucla, and Coolgardie, approximately similar to the line on which the longitude-signals were sent, returned to the observatory, and recorded clock-beats on the chronograph, side by side with those recorded directly. The difference in time between the two sets of taps, giving the time of transmission on the telegraph-line, was $0^{s}\cdot27$, which accorded well with the result above mentioned.

Few details are at present given as to the determination of clock-errors. The error of a night depends on the observations of less than a dozen stars at each station, which seems few compared with the number of signals. The results for the five nights have a range of $0^{s}\cdot18$, so that Mr. Cooke may be congratulated on a fair accordance in his results; but there is always some doubt whether it is possible to find *absolute* local time with a non-reversible transit, and the method of eliminating personal equation (Mr. Cooke and Mr. Griffiths determined their relative personal equation with the Adelaide instrument) is open to objection, so that the result can scarcely be considered as final, which no doubt Mr. Cooke does not intend it should be.

The latitude was determined from meridian zenith-distances of circumpolar stars. The results on five days, an average of 10 stars being observed at each culmination, show a range of $0''\cdot50$, the mean being $31^{\circ}\ 57'\ 9''\cdot63$ after an approximate correction for flexure is applied. But this result is evidently considered as provisional, since no correction for errors of division has yet been applied.

PUBLICATIONS.

A Treatise on Instruments *.—Despite the proverbial objection to comparisons, it is often convenient, in estimating value, to have some standard with which to strike a mental balance. At this moment, casting around for such a standard, we can think of nothing with which to compare Dr. Ambronn's voluminous work now before us. This is an encyclopædia of astronomical instruments. It consists of two volumes, 1262 pages in all, divided into seven chapters. It is written in German, but this circumstance is mitigated, so far as concerns readers not very familiar with the language, by the fact that it is not in German type, and that there are 1185 pictures, which materially help to throw light on the text.

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* 'Handbuch der astronomischen Instrumentenkunde.' Von Dr. L. Ambronn. Mit 1185 in den Text gedruckten Figuren. Julius Springer, Berlin, 1899. Price £3.

The first volume may be said almost to consist of description of parts which go to make up an instrument. It opens with a chapter on screws, first simple screws, then combinations for adjustment and for locking; among these last is an arrangement, not often met with, of a right- and left-handed thread on the same shaft and on the same part of it. Nuts with right-handed and left-handed threads can both turn on this, but when two such are in contact they lock, as the friction between them prevents them turning in opposite directions. The chapter then goes on to micrometer-screws, and as an example of the fact that the work is more than a catalogue of instruments, but rather a treatise on their theory, it may be stated that examples are given of the periodic errors of screws and the method of their determination.

Levels and their forerunner, the plumb-line, form the subject of the next chapter. The level-prover is described, but there is no mention of the levels in use at Greenwich, which are virtually level-provers. The suggestion by Dr. Common in 'Monthly Notices,' November 1891, might also have found a place in this chapter. In this section also there is noted the method of setting a transit instrument by means of a level, but we have not noticed in the volumes the extension of this principle to the equatorial, lately noticed in this magazine in writing of Sir H. Grubb's catalogue; but this mention of these small omissions must not be taken as adverse criticism, must rather be taken to imply that there is so much worth mentioning that it is easier for the critic to note the omissions; but among the excellent things which are in the book are chapters on Axes, which comprise descriptions of wyes and bearings—we do not notice any diagrams of sector-bearings—and methods of determination of pivot-error.

There is a chapter on circles, which includes dividing-engines and the theoretical formulæ relating to and examples of eccentricity and determination of division-error. As an instance of the small things given in the work there may be noted a hint to be followed in fixing spider-webs in a telescope. Wet the webs, and fix them whilst wet, they will then be less liable to afterwards sag in damp weather.

In the chapter on Telescopes the diagrams of imperfect star-images familiar to us in Messrs. Cooke's book on Objectives are given. The triple-objective, which is the speciality of the York firm, is of course noted.

The first volume contains also a chapter on Clocks and Chronometers, of which it may be said that it is full; but the methods described are essentially German; there is no mention of any of the early English clock-makers, nor do the names Dent or Kullberg appear in the index, but Bréguet is found there.

The second volume, which contains descriptions of completed instruments, is difficult to summarize. There is a chapter on micrometers, giving all forms from the earliest form of scale-micrometer to the latest complicated form by Messrs. Repsold, with the addition of all kinds of double-image micrometers including the heliometer. Prof. Kapteyn's "equatorial" micrometer for measuring astrographic plates is treated fully, but other forms for this purpose have less attention. The next chapter, on photometers, is especially valuable, as it collects all varieties of this instrument, which is probably as little known generally as any astronomical instrument. The chapter on spectrum-apparatus is perhaps the least complete in the work. If we judge merely by quantity forty-four pages seems comparatively a small allowance, and of these the object-glass prism occupies only four.

The latter part of the work might be described as a catalogue of all the principal instruments that have ever existed. The Altazimuths and Vertical Circles occupy a chapter of 90 pages, with a picture of an instrument on almost every page, among them being the Poulkova Vertical Circle, the Greenwich Old Altazimuth and New Universal Transit, Dr. Chandler's Almucantar and the Chronodeik, with examples of results obtained from it.

There are examples of Transit Instruments from Romer's to that made by Salmoiraghi for the Observatory of the Collegio Romano. The book is published a little too soon to include the new Cape Transit Circle, which is to be regretted; but in addition there is a section on transit instruments of special construction, such as Troughton's Transit, in which the telescope is fastened between a pair of braced circles of diameter almost equal to its length: the method proposed by Dr. Common * (we do not know whether this has ever been realized) is also represented. This chapter ends with instructions and the usual formulæ for the use of a transit, and, moreover, includes the cognate subjects of personal-equation machines and chronographs

The Chapter on Equatorials is equally complete; it would take too much space to name all the illustrations given. They include all the well-known instruments, from Sisson's (1775), which is not quite the earliest, however, to the Paris Coudé and the new Cambridge instrument, and the volume ends with plans of Observatories and Domes. There are shutters of all shapes; but the shutterless dome, a neat form of which has lately been designed by Messrs. Cooke, is not represented.

It is difficult to show all the points of two such volumes as

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* Mon. Not. R.A.S. vol. xliv. p. 288.

these within limited space, but it is hoped that this brief account has made evident the character of Dr. Ambronn's work—an encyclopædia which should find a place on the reference shelves of every astronomical library.

NOTES.

Comet Notes.—We learn from 'Publications of the Astronomical Society of the Pacific,' No. 70, that Comet 1898 VII. (Coddington) was followed at the Lick Observatory till Sept. 7 last, a period of 15 months. Its distance from the Sun was then 4·46, and its brightness estimated as 15th magnitude.

The same publication states that the correction required by Zwiers' revised ephemeris of Holmes' Comet in August last was $+0^{\mathrm{s}}{\cdot}66$ in R.A., $+9''{\cdot}2$ in Decl.

The comet was fainter than the 14th magnitude, with slight central condensation, but no true nucleus; its outline was very vague.

Comet Tempel II. was an easy object in a small telescope in July, and was still of the 10th magnitude on Sept. 26, having a nucleus of the 13th magnitude. The comet is still under observation at the Lick Observatory.

The 'Monthly Notices' (Supplementary Number) contains some interesting physical observations of this comet, made at Grahamstown by Major Eddie. On August 15 the head was sharply defined, with central condensation; faint, owing to moonlight.

On August 24, 25, 28 a faint tail could be traced for 16′; it had a dark rift in it behind the nucleus. The latter was 2′ in diameter, surrounded by a large diffused coma.

The following are the latest elements of Giacobini's Comet (*e* 1899). They are by S. K. Winther from observations on Oct. 1, 9, 22 (Ast. Nach. 3600):—

T ……	1899 Sept. 13·9178 Berlin M.T.	
ω ……	10° 9′ 54″	1899·0.
☊ ……	272 16 12	
i ……	77 3 8	
log *q* ….	0·25116	

Ephemeris for Berlin midnight.

	R.A.			N. Dec.	
	h	m	s	°	′
Dec. 2….	18	7	36	12	47
6….	18	14	24	13	56
10….	18	21	17	15	5
14….	18	28	13	16	16
Dec. 18….	18	35	14	17	29
22….	18	42	17	18	43
26….	18	49	25	19	59
30….	18	56	37	21	16

Brightness on Dec. 2 0·45, on Dec. 26 0·35, that at discovery being unity.

On Oct. 23 the comet was observed by Prof. Hartwig at Bamberg, and is described as round, 1′ in diameter, total light equal to an 11th-mag. star (Ast. Nach. 3601).

The 'Astrophysical Journal' for October contains an article by W. H. Wright on observations of comet spectra. Comet 1898 I. (Perrine), 1898 VII. (Coddington), 1898 X. (Brooks) were all of the usual type, viz., faint continuous spectrum with the three characteristic bright bands. The continuous spectrum in the third of these was, however, very weak, as was also the case in the spectrum of 1899 I. (Swift), which consisted almost entirely of bright lines. A correcting-lens was placed in front of the slit, and some very satisfactory photographs of the spectrum secured. The three carbon bands, and possibly a fourth and fifth, are shown. Others are considered to be due to cyanogen.

Ast. Nach. 3602 contains the following hyperbolic elements of Comet 1899 I. (Swift), computed by C. J. Merfield from 59 observations extending from 1899 March 4 to July 12:—

T ……	1899 Apr. 12·97772 Greenwich M.T.
ω ……	8° 41′ 55″·7
☊ ……	24 59 18 ·3
i ……	146 15 27 ·7
$\log q$ ….	9·513975
e ……	1·000395

The same publication contains further details of Mr. Perrine's observations made on May 18 and June 9 last, to examine whether the refraction produced by this comet appreciably shifted the position of stars over which it passed. The answer is in the negative.

There is also an article by Dr. Schobloch on the resemblance of the orbits of Comets 1881 IV. and 1898 X. Both orbits make fairly close approaches to that of Jupiter, and he suggests that the comets may have originally had a common elliptical orbit, which has now been transformed into a sensibly parabolic one by the action of Jupiter. A. C. D. C.

Minor Planet Notes.—Two new planets, EU, EV, were discovered by Wolf and Schwassmann at Heidelberg on Oct. 31, and another, EW, on Nov. 4. It appears that EV had been already photographed by them on Oct. 10 and 26, but was at first supposed to be identical with (214) Aschera. This identity is now definitely disproved (Ast. Nach. 3601).

Another planet, EX, was discovered by Coddington at the Lick Observatory on Oct. 2, but the discovery was not announced till a month later. A. C. D. C.

A remarkable Series of Azimuth Errors.—It is the practice at Greenwich, as no doubt at other places, to observe Polaris both at upper and lower culmination whenever the clouds permit, except at the very inconvenient hours of the night, to determine the azimuth error of the transit-circle. It seems worth putting on record that between Oct. 6 and 19 last the observation of every culmination was obtained except two—26 in all. The combination of these in consecutive pairs gave 23 errors, which ranged between $11''\cdot32$ and $12''\cdot98$, with no greater difference than $0''\cdot78$ between any consecutive two. This is to be regarded, not so much as testimony to the general absence of cloud during the period, but rather to the steadiness of the instrument.

Jupiter's Satellite V.—Prof. E. E. Barnard has secured a most important series of measures of Jupiter's Fifth Satellite. The observations, which were made with the 40-inch Yerkes refractor, extend over the period 1898 March to 1899 June, and are published in Ast. Journal, No. 472. A feature of the measures is the time each evening over which the measures are extended. Thus on 1899 May 1 the first measure was made at $11^h\ 56^m$, and the last of 37 at $13^h\ 52^m$, during which time the distance of the satellite had changed 10 seconds of arc ($30''\cdot9$ to $20''\cdot4$) as measured from the limb of Jupiter.

Harvard Observatory Finance.—Prof. E. C. Pickering has lately circulated a little pamphlet giving some facts about the pecuniary resources of the Harvard College Observatory. It seems that the Committee in the past have been most successful beggars. Their efforts in this direction have been rewarded in the most magnificent way, but still Prof. Pickering asks for more, mainly for the reason that owing to the decrease of the rate of interest on money, though the capital of the institution has largely increased in the last twenty years, the income is not sufficient for the Observatory needs.

In 1877 the capital was $173,908, which has been augmented by

	$
(1) A sum collected by the Committee annually for five years	5,000
(2) A subscription raised by the Committee	50,000
(3) Mr. R. T. Paine's bequest	323,558
(4) Miss Bruce's gift	50,000
(5) The Haven bequest, and others	45,000
(6) A subscription raised by the Committee	15,000
(7) Mrs. Draper's annual gift	10,000
(8) The Boyden bequest	Amount not stated.

with the result that the invested capital is now $825,699, and the annual receipts of the Observatory $46,175 as against $14,360 in 1877.

The Director, after describing the scientific results which have been arrived at by the use of the money at his command, points out the necessity for an increase in the resources of the Observatory, and suggests that the attention of the public be called to the fact that it is desired to increase the capital of the Observatory by $200,000, to compensate for the loss of income due to falling off in the rate of interest. If this sum cannot be obtained, the sum of $50,000, if expended during the next ten years, would provide for the reduction and publication of a large part of the material now in hand.

THE SYSTEM OF 7 TAURI (Σ 412).—This system is now worthy of special attention by possessors of large telescopes. The two stars which form the close pair are of magnitude 6·6 and 6·7 respectively, and undoubtedly form a binary system whose period is about 250 years. Since 1827 the star B has moved, with respect to A, over an arc of 73°, but has now arrived at a portion of the orbit where the annual decrease of position-angle is 3°. The distance will not exceed 0″·3 for many years. The following is a rough ephemeris :—

1900	198°	0″·30	1910	172°	0″·28
1905	178	0 ·29	1915	155	0 ·27

The small star C is about the 10th magnitude and somewhat bluish, A being white and B a pale yellow. This third star has a motion of 0″·013 in position-angle 0, a value smaller than the proper motion of A, but in exactly opposite direction. It may therefore be considered as fixed and in no way connected with the system AB, but might be useful for parallax observations. The position of C is 60° and 22″·2.

R.A. of 7 Tauri, $3^h\ 28^m\ 30^s$; N.P.D. 65° 52′.

T. L.

“AT the Board of Trade on Nov. 15 Sir Courtenay Boyle presided over a Conference of representatives of electrical railway and electrical tramway enterprises on the one hand and representatives of the Government interested in the Greenwich and Kew Observatories on the other. The object of the Conference was to ascertain the best means of dealing with the alleged interference with the delicate instruments in the observatories by the leakage which it is assumed will follow from the introduction of large systems of electrical traction. After Sir Courtenay Boyle had opened the proceedings, interesting speeches were delivered by Professor Rücker and the Astronomer Royal, who alleged that there must be magnetic disturbance; and by Sir Douglas Fox, Major Cardew, and others, who would not admit that any interference with observatory instruments had at present been proved. As a result a Committee was appointed 'to investigate the amount of magnetic disturbance produced in the neighbourhood of electrical tramways and railways constructed and worked under the Board of Trade regulations, and to report as early as possible.' "—*The Standard.*

As the year draws to a close calendars and ephemerides for the year 1900 come to hand. We have received the Royal Navy List, Diary, and Naval Handbook from Messrs. Wetherby & Co., which we can recommend as a useful addition to the desk or reference library, containing, as it does, not only the usual almanac and astronomical ephemerides, but also a quantity of information both of a scientific and general nature. We have also Mr. Arthur Mee's card-almanac, 'The Heavens at a Glance,' which will doubtless prove no less useful than its predecessors.

It is announced that Mr. R. H. Scott, the Superintendent of the Meteorological Office, will retire at the end of the present year, after thirty years' service, and that Mr. William Napier Shaw, of Emanuel College, Cambridge, University Lecturer on Experimental Physics, has been recommended by the Meteorological Council as his successor.

The second volume of Dr. Isaac Roberts's 'Photographs of Stars, Star-Clusters, and Nebulæ' is about to be issued from the publishing office of 'Knowledge.' It will contain seventy-two photographs beautifully reproduced by the collotype process, in addition to many pages of text. Only a limited number of copies of the work will be available for the public.

At the meeting of the Leeds Astronomical Society on October 25 Mr. Washington Teasdale, Past President, gave an interesting lecture on the subject of Stonehenge and the astronomical theories relating thereto. In the 'Weekly Yorkshire Post' of November 4, the hypotheses brought forward at various times, which the lecturer described, are given, and also an addendum by Mr. Whitmell on the age of Stonehenge.

Obituary.—We regret to hear of the death of Mr. Nathaniel E. Green, sometime President of the British Astronomical Association. We hope to give some account of the work of this well-known astronomer in our next number.

An eclipse of the Moon, nearly total, and visible at Greenwich, is predicted to happen on December 16. The circumstances of the phenomenon, and a list of small stars which will be occulted, are given in the 'Companion' for 1899. Neptune will be occulted shortly after the end of the eclipse.

The next Meeting of the Royal Astronomical Society will take place on December 8; of the British Astronomical Association on December 27.

FROM AN OXFORD NOTE-BOOK.

THE Report of the Madras Observatory for 1898–99 contains the following paragraphs, which are of sufficient importance to reproduce *verbatim*, for there are many readers of this magazine who will not see the Report itself:—

> 6. *Kodaikanal Observatory.*—As mentioned in last Report the Government of India requested the Astronomer Royal and Sir Norman Lockyer to report on the various Indian observatories. The former, after visiting Kodaikanal, approved generally of the plans for the observatory there, and made some suggestions for minor alterations which were at once adopted. Sir Norman Lockyer, on the other hand, without visiting the place, objected entirely to the plans, and on his return to England represented to the Secretary of State for India that the buildings were "too costly and too permanent," and generally were badly designed and unsuited for their purpose. He went on to point out that "the South Kensington Solar Physics Observatory thus equipped with temporary structures is the most powerful in the world. It does more and better work than the similar institution at Potsdam, where the buildings cost £250,000," and urged that the new buildings at Kodaikanal should be like those at South Kensington, "shanties" built of wood and canvas. As a consequence, the Secretary of State telegraphed out that the work on the observatory was to be stopped till the reports of the Astronomer Royal and Sir Norman Lockyer had been duly considered.
>
> To anyone acquainted with the climatic conditions existing at Kodaikanal the proposal to house valuable instruments in such "shanties" as Sir Norman Lockyer recommended seems as strange as his estimate of the relative value of the work done at South Kensington and Potsdam, and the Government Astronomer protested strongly against his proposals. Whether or not this protest was forwarded to the Indian Observatories Committee is not known, but the result of the deliberation of the committee was that no reference whatever was made to the buildings, and after a delay extending from the beginning of June to the end of October, the buildings were allowed to go on according to the designs which had been so strongly condemned. The stopping of the work for so long naturally led to further delay, for not only was the favourable season for building lost and the staff of workmen dispersed, but the Assistant Engineer in charge of the work was given the superintendence of other works in Madura, which necessitate his frequent absence from Kodaikanal and the consequent delay of the work there. Hence the buildings which would have been finished before the end of the year are still far from completion.

THERE is no doubt that much good work can be done in "shanties" when nothing better is available; indeed, the work is sometimes so good that it is often the "shanty" which is "too permanent," and remains long after it should have been replaced by a decent building. There were, until lately, a number of such shanties at the Royal Observatory, Greenwich, which were never intended to be more than temporary expedients, but lasted far longer than they should, in spite of the efforts of the Astronomer Royal to get them replaced, which have only recently been crowned with success. The magnetic house is still there oriented in a manner which bears witness to the short life originally intended for it. But I imagine that most men who can afford it prefer to live in a house of bricks and mortar rather than a hut; and climatic conditions sometimes make this imperative. It is a pity that a man who keeps himself so much before the public as

Sir Norman Lockyer should overstate his case in a way which is certain to be misunderstood. He would appear to be directing a sort of crusade against special adaptation to the purpose both in buildings and observers, to judge from his extravagant remarks on the occasion of the last eclipse expedition. If he prefers as observers a ship's crew with a week's training, and as observatories a few shanties, by all means let him have them; but why make it difficult for others, who have not these peculiar ideas, to get what they want?

AMONG numerous book catalogues which reach one at different times, that recently published by Messrs. W. Wesley & Son (Essex Street, Strand) is deserving of special attention. Their Circular No. 135 is a catalogue of more than 2000 astronomical books and pamphlets—"the literature since 1800." The Library of the R.A.S. contained, at the time of the last printed Catalogue, 9000 volumes, half of which belong to series of publications, and many of which are of course of earlier date than 1800; so that the Catalogue of Messrs. Wesley & Son represents a considerable fraction of the literature dealt with. A scheme of classification is attempted with a considerable measure of success; only those who have tried it know how difficult it is to classify astronomical literature. Shall a Star Catalogue, for instance, come under "Publications of Societies and Observatories" or under "Stellar Astronomy"? Messrs. Wesley choose the latter. Are observations of Mars at Ascension to come under "Solar Parallax" or under "Mars"? They choose the former. Of course in many cases cross-references would diminish the importance of such selection.

THE Leonids were again a great disappointment. Either they have left us altogether or they are considerably late at the cross-roads. Or are they a ring or spiral which we passed through the middle of? The only observers who have some little cause for satisfaction are those who, like Mr. Denning at Bristol, had two cloudy nights: they must be rather relieved to find that they missed nothing. On the early morning of Wednesday, November 15, it was brilliantly fine at Oxford, and we saw 50 or 60 Leonids; but there was no sensational display. Several disappointments have been expressed in verse—as, for instance, in the *Daily News.* I give here, by kind permission of the Editor of the *Oxford Magazine,* another poetical lament, which fewer of our readers may have had the chance of seeing. In explanation of the third verse, I may mention that in order to give due warning to the Colleges in case of a display, I suggested that one member of each College should pass the two nights at the Observatory, sleeping as best he could on the floor of the large lecture-room with cushions and rugs brought by himself. In the event of a display these representatives would be awakened to carry the news to their Colleges. The proposal was not inviting to those fond of comfort, but it met with enthusiastic response from about 40 people. I had suggested one from each College, but found it impossible to refuse the earnest pleas of those who wished to be supernumeraries. They did not all sleep; many of them watched as anxiously as we did, or asked all sorts of questions of Mr. Bellamy. I think he quite enjoyed seeing the Dome so well filled with company, in contrast to its usual appearance. I may add that some of those who did sleep, in spite of the aching sides, were very hard to awaken; we overlooked one sound sleeper at sunrise on Thursday morning and nearly left him locked in. Also many of those who had gone through the discomfort for two nights were anxious to come back on a third! and with the greatest reluctance accepted my assurance that their chance was gone for this year. But here is the poem:—

The Unpunctual Leonids.

We waited for our friends to come—
  Exceptionally brilliant guests;
But though we gave a grand "At Home,"
  They disregarded our requests.
They 'll come just when their fancy bids—
The Leonids, the Leonids!

We wooed them here with all our charms;
  We breathed to them our fondest hopes;
We welcomed them with open arms,
  With cameras and telescopes.
Greenwich had spent a thousand "quids"
Preparing for the Leonids.

Our own Professor was not slack;
  He housed three dozen youths and more,
With aching sides and weary back,
  On the Observatory floor.
But sleep fell on their heavy lids
Before they saw the Leonids.

Have the South-Western's evil ways
  Degraded Kepler's rigid laws?
Have meteors learned our railway phrase?
  Has "heavy traffic" been the cause
Which checks the streaming shoal that thrids
The orbit of the Leonids?

What planet has disturbed their course,
  And thrown such disrespect on us?
Has Saturn used excessive force?
  Or was it distant Uranus
That brought retarding brakes and skids
To stop the unpunctual Leonids?

At any rate, we hold it best
  To waste no more November hours:
We shall transfer our interest
  To August's meteoric showers,
And the neglected Perseids
Shall dispossess the Leonids!

Astronomers are blank with shame,
  All their prognostics came to grief!
An expletive I will not name
  Would give each scientist relief.
We feel that Decency forbids
All mention of the Leonids.

At the moment of going to press I have received a cutting from a Newcastle newspaper which deals severely with astronomers for having deluded the public:—

> The whole thing has been very cleverly worked. The agency of the whole press of the United Kingdom has been enlisted in a grand attempt to delude the populace. Eminent astronomers have written pointing out exactly when and where the meteors may be expected to appear. Thousands of sober citizens who give no other sign of insanity have spent hours on succeeding nights in endeavouring to locate the exact spot in which the meteors would be seen, and have gone to bed with a bad temper, a cold in the head, and a crick in the back of the neck. Meanwhile, no doubt, the comfortable bedrooms at Greenwich Observatory were rocking with the laughter of the men who had taken in a nation. No doubt they can afford to laugh now, but let them wait a while.

The writer of this paragraph has taken an ingenious and adequate revenge at any rate. If astronomers invented the meteors, he has invented the bedrooms. It was probably as well known to him that there were no bedrooms to rock with laughter as it was to astronomers that there were no meteors to fall; so after all we may cry quits and ask him to forego the terrible revenge darkly foreshadowed in his closing words.

The following paragraph has been sent me by a correspondent. It is a cutting from some newspaper, but has no name or date attached:—

THE FLAT-EARTH MAN.

Transvaal Folk recognize his Merit.

> The patience and long-suffering of Mr. Ebenezer Breach, the philosopher who maintains that the Earth is flat, have at length had their reward. He has received a letter from Johannesburg announcing that he has been elected "Grand Master of the Ancient Order of Unshaven Boers."
>
> But previous to his taking office he has to fulfil two conditions—the first that he shall don the regalia of the order, namely, yellow breeches and red embroidery; and the second that he shall apply to the magistrates for a certificate of identification.
>
> Armed with the letter he yesterday appeared before the Portsmouth magistrates, and asked them to sign a document which ran:—"This is proof that Ebenezer Breach is a resident and burgess of the borough of Portsmouth."
>
> The bench told him that he should have put his photograph on the document, in which case they probably might have signed it.
>
> Mr. Breach tried to argue the point, but in the end he withdrew, declaring that he would comply with the suggestion and apply again.
>
> It may be remembered that some time ago Mr. Breach wrote to Sir John Gorst threatening to put in force against him an old statute which is said to have made it illegal to teach in schools that the Earth is round. To this letter he has not had a reply.

~~DOES NOT CIRCULATE~~

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